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VOL. III.

FIRST MEMOIR.

THE SUFFICIENCY OF TERRESTRIAL ROTATION FOR THE DEFLECTION OF STREAMS.

THE SUFFICIENCY OF TERRESTRIAL ROTATION FOR THE DEFLECTION OF STREAMS.

By G. K. Gilbert.

Read April 15, 1884.

It was long ago perceived that rivers of the northern hemisphere flowing to the north or to the south should by the rotation of the earth be thrown severally against their east or west banks. It is even many years since it was shown by Ferrel that these tendencies are but illustrations of a more general law, that all streams in the northern hemisphere are by terrestrial rotation pressed against their right banks, and all in the southern are pressed against their left banks, the degree of pressure being independent of the direction of flow. Yet the question of the sufficiency of the cause for the production of observable modifications in the topography of stream valleys is still an open one. A number of geologists have observed peculiarities of stream valleys which they referred to the operation of the law, while others, including myself, have looked in vain for phenomenal evidence of its efficiency. Nevertheless it is my present purpose to maintain the sufficiency of the cause.

So far as I am aware, all those who have attempted to consider analytically the mode in which the lateral tendency arising from rotation should modify the channel or valley of a stream, have reached the conclusion that no appreciable results can be produced, and for the most part their conclusions legitimately follow their premises.* My own different conclusion is based upon an essentially different analysis of the processes involved. In the celebrated discussion of the subject in the French Academy of Science, it was computed by Bertrand that a river flowing in 45° north latitude with a velocity of three meters per second exerts a pressure on its right bank of $\frac{1}{63539}$ of its weight, and he regarded this pressure as too small for consideration.† It has been pointed out by Henry Buff that the deflecting force, by combining with gravitation, gives the stream's surface a slight inclination toward the left bank, thereby increasing the depth of water near the right bank, and consequently increasing the velocity of the current at the right. To this increment of velocity he ascribed a certain erosive effect, but regarded it as less than that assignable to wind-waves on the same water surface. He therefore accorded a more important influence to the prevailing winds than to the rotation of the earth.‡ It has been held by others that the combination of the deflective force with gravitation is equivalent simply to a slight modification—so far as the stream is concerned—of the direction of gravitation; and that, the flood-plain of the stream having been adjusted normal to this modified direction of gravitational attraction, no other geological effects are produced. The last was my own view until I perceived the importance of certain considerations, to which I now proceed.

The form of cross-section of a stream flowing in a straight channel depends on the loading and unloading of detritus, and is essentially stable. It is evident that the form of the cross-section

* I have recently become cognizant of a discussion by Baines to which this sentence does not apply. See note to the following page.

† Comptes rendus, XLIX, 1859, p. 658.

‡ Annalen der Chemie u. Pharmacie, IV Supp. I Band. Leipzig and Heidelberg, 1865-1866.

controls the distribution of velocities of current within its area, and that through the interactions of these velocities its parts are interdependent. Each element of its curve is so adjusted to the adjacent current and to the detrital load of the stream that it can neither be eroded nor receive a deposit, and the stability of the profile depends on the fact that an element not adjusted to the contiguous current and load becomes subject either to erosion or to deposition until an adjustment is reached. The distribution of velocities within the cross-section is symmetric, the swiftest threads of the current being in the center and the slowest adjacent to the banks.

If, now, curvature be introduced in the course of the channel, centrifugal force is developed. This centrifugal force is measured by the square of the velocity, and is therefore much greater for swift central threads of the current than for slow lateral threads. As pointed out by Thomson* and others, the central threads, tending more strongly toward the outer bank, displace the slower threads of that bank, and the symmetry of distribution of velocities is thus destroyed. In other words, the centrifugal force developed by curvature exercises a selective influence on velocities, and transfers the *locus* of maximum velocity from the center of the channel toward the outer bank. The conditions of symmetry in the profile of the cross-section are thus destroyed: the outer bank is eroded; a deposit is accumulated on the inner bank. Moreover, there is no compensating tendency to restore an equilibrium, for the erosion of the outer bank increases the sinuosity of the channel instead of rectifying it.

Curvature of course thus causes a stream to shift its channel laterally, and in this manner enlarge its valley. It is the most important condition of lateral corrasion.

As shown by Ferrel, the deflective force due to terrestrial rotation varies directly with the velocity of the stream. Therefore, it likewise has a selective influence on the velocities within the cross-section of the channel; and it likewise tends to produce erosion at one side and deposition at the other.† For given amounts of deflective force its selective power is not the same as that of the centrifugal force developed by curvature of course, for centrifugal force varies with the second power of the velocity, while the rotational deflective force varies only with the first power; but its selective power is of the same kind, and may be quantitatively compared. For the purpose of this comparison I will develop an equation:

Let $F=$ deflective force, per unit of mass, due to rotation.
$n=$ angular velocity of the earth's rotation.
$v=$ velocity of stream.
$\lambda=$ latitude of the locality.
$\rho=$ radius of curvature of the stream's course.
$f=$ the centrifugal force, per unit of mass, developed by such curvature.

Then

$$f=\frac{v^2}{\rho} \quad \text{. . . . . (1)}$$

and, from Ferrel,‡

$$F=2vn \sin \lambda \quad \text{. . . . . (2)}$$

Let $v_r=$velocity of a rapid-flowing thread of the current, and
$v_s=$velocity of a slow-flowing thread of the current.

Represent by F_r, F_s, f_r, and f_s the corresponding deflective forces due to rotation and curvature, then

$$F_r-F_s=(v_r-v_s)\times 2\,n \sin \lambda \quad \text{. . . . . (3)}$$

and

$$f_r-f_s=\frac{v_r^2-v_s^2}{\rho} \quad \text{. . . . . (4)}$$

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* Trans. Brit. Ass., 1876, Sections, p. 31.

† This proposition, which it is the prime object of the present paper to set forth and develop, was believed, at the time it was read, to be novel, but proves to have been anticipated by more than six years. In October, 1877, Mr. A. C. Baines read before the Philosophical Institute of Canterbury, New Zealand, a paper "On the influence of the earth's rotation on rivers," in which he arrived, by a very different route, at essentially the same conclusion. See Trans. N. Zeal. Inst., X, pp. 92–96.

‡ Ferrell's equation is given on page 29, volume 31 (second series), Am. Jour. Sci. Instead of the sine of the latitude, here substituted, it includes the cosine of the polar distance, which is, of course, equivalent.

$F_r - F_s$ evidently expresses the selective power due to rotation, and $f_r - f_s$ similarly expresses the selective power due to curvature. Where the curvature has a convexity to the right, these two influences conspire, and their resultant is deducible by addition. Where the curvature has a leftward convexity, the influences are opposed, and their resultant is deducible by subtraction. [The terminology here and throughout the remainder of the paper is adjusted to the northern hemisphere exclusively.]

If we represent by R the joint selective power on curvatures of right-hand convexity and by L the joint selective power on curvatures of left-hand convexity, then we deduce, by simple combinations and transformations of equations (3) and (4),

$$\frac{R}{L}=\frac{v_r+v_s+2\ \rho n\ \sin\lambda}{v_r+v_s-2\ \rho n\ \sin\lambda} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (5)$$

v_r and v_s may be the velocities of any two threads of current moving at different rates, but for purposes of convenience and simplification we now assume that they are symmetrically related to the mean velocity v; and introducing this relation in (5) we obtain

$$\frac{R}{L}=\frac{v+\rho n\ \sin\lambda}{v-\rho n\ \sin\lambda} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (6)$$

This equation expresses the ratio between the selective influences tending to determine the maximum velocity toward the right and left banks respectively of a meandering stream. Since these tendencies result in erosion, their ratio is a function of the tendency of a stream to erode its right bank as compared with its tendency to erode the left.

For the purpose of quantitative illustration, the Mississippi River will be considered. In its lower course the sharper bends have a radius of curvature, measured to the center of the channel, of about 8,000 feet. These curves, together with all other channel features, are determined by the water at its flood stage. It is therefore proper to consider in this connection the mean flood velocity. That was determined by Humphreys and Abbot to be, at Columbus, Ky., 8.4 feet per second.* The latitude of the locality is 37°. Giving these values to ρ, v, and λ, and substituting for n its numerical value, .000072924, we obtain from (6).

$$\frac{R}{L}=1.087.$$

The selective tendency toward the right bank is therefore nearly 9 per cent. greater than toward the left.

With the elements of another stream it is probable that a very different result would be obtained; but this single example suffices to show that while the influence of rotation is small as compared to that of curvature, it is still of the same order of magnitude, and may reasonably be expected to modify the results of the more powerful agent. In the present state of hydraulic science it is impossible to define the quantitative relation between the tendency of swift threads of current toward a bank and the consequent erosion; but whatever that relation may be, I conceive that rotation is competent to produce appreciable results wherever those due to curvature are great.

It will be observed that the efficiency of rotation thus advocated is only in connection with, and as an adjunct to, lateral wear by means of curvature. There are two general cases, including a large share of all streams, to which the conclusion does not apply: (1) A stream which rapidly corrades the bottom of its channel does not notably corrade its banks; and in such case the effect of rotation should not be discoverable. (2) A stream engaged in the deposition of detritus, as on a delta or an alluvial fan, shifts its channel from side to side by a process entirely distinct from the one just described. It builds up its bed until it is higher than the adjacent plain, and then transfers its current bodily to a different course. Rotation has its share of influence in determining the direction of this transfer; and it thereby induces the stream to build its alluvial plain higher on the right than on the left; but, the difference of level having been established, the stream has

*Humphreys and Abbot, Report on Mississippi River, p. 595.

thereafter no more tendency to one side than the other. Deflective effects of rotation are there fore not to be sought in regions of alluvial deposition.

It may be remarked also that the tendency of a stream toward one bank or the other by reason of curvature and rotation is often overpowered by an opposite tendency due to obstructions. These include resisting members of the eroded terrane, and alluvial dams deposited at one bank or the other by tributaries.

A general curvature in the course of the valley through which the stream flows has the same tendency as does the curvature of a short bend, only in a less degree; and this tendency must in many instances nullify or conceal the results of rotation.

Visible examples of the work of rotation are therefore to be sought especially in streams which, with courses in the main direct, are slowly deepening their valleys by the excavation of homogeneous material. The best locality of which I have knowledge is one to which attention was called by Mr. Elias Lewis, in the American Journal of Science for February, 1877, and which has recently been visited at my request by Mr. I. C. Russell. The south side of Long Island is a plain of remarkable evenness, descending with gentle inclination from the morainic ridge of the interior to the Atlantic ocean. It is crossed by a great number of small streams, which have excavated shallow valleys in the homogeneous modified drift of the plain. Each of these little valleys is limited on the west or right side by a bluff from 10 to 20 feet high, while its gentle slope on the left side merges imperceptibly with the general plain. The stream in each case follows closely the bluff at the right. There seems to be no room for reasonable doubt that these peculiar features are, as believed by Mr. Lewis, the result of terrestrial rotation. As the streams carve their valleys deeper they are induced by rotation to excavate their right banks more than their left, gradually shifting their positions to the right, and maintaining stream cliffs on that side only.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SECOND MEMOIR.

ON THE TEMPERATURE OF THE SURFACE OF THE MOON.

ON THE TEMPERATURE OF THE SURFACE OF THE MOON.

FROM RESEARCHES MADE AT THE ALLEGHENY OBSERVATORY BY S. P. LANGLEY, ASSISTED BY F. W. VERY AND J. E. KEELER.

Read October 17, 1884.

From the earliest ages it has been observed that the moon's rays bring us light, but no sensible heat. When, in the course of time, the phenomena of nature began to be subjected to more exact scrutiny, it was seen that in view of the very obvious brightness of the moon, the absence of heat in its rays was an anomalous circumstance, and in the last century Tschirnhausen, La Hire, and others, with the largest burning lenses or mirrors, and the most delicate thermometers of that time, attempted to obtain indications of heat, but without success. As apparatus improved in delicacy, it began to be noticed that (on the contrary) indications of actual cold were often obtained when the thermometer lay in the focus of burning mirrors which concentrated the rays of the moon on it; its concentrated heat, if any existed, being so nearly *nil*, as to be overbalanced by the increased radiation of the thermometer toward space or to the substance of the mirror itself. Other observers, like Howard,* fancied they obtained signs of heat with sensitive thermometers, but these were doubtless due to inexperience of the precautions necessary in eliminating the effect due to the radiations from the apparatus itself, radiations which may give delusive indications of marked lunar heat or cold, according (for instance) as the screen withdrawn be itself colder or warmer than the thermometer by some immeasurably small fraction of a degree. We can hardly overstate the probability of error in such a research in any hands but those educated to multiplied precautions, such as were used by Professor Forbes,† who, employing a lens by which the lunar heat was concentrated about 6,000 times, still obtained no certain evidence of heat. He was able, however, to conclude from this negative result that the warming effect of the full moon on the surface of the earth would at any rate not exceed $\frac{1}{300000}$ of a degree Centigrade.

The first satisfactory evidence of actual heat was obtained by Melloni,‡ who, with a polyzonal burning lens of one meter aperture and one meter focus, with the newly invented thermopile, in the clear air of Vesuvius, after due precautions against instrumental error, was enabled to announce that indications of heat had been obtained, though the effect was still all but immeasurably small. Prof. Piazzi Smyth, § upon the Peak of Teneriffe, obtained also some apparent indications of heat, but all these measures, and a large number of others which I do not cite, including those of such skilled observers as Tyndall ‖ and Huggins,¶ lead only to the conclusion that the moon's heat is so small that we can do little more than detect its existence, though M. Marie Davy** a little later obtains some apparent evidences of the change of heat with phase, indicating a direct effect of about $\frac{1}{1000000}$ degree for the full moon, which he observes is about the one-fiftieth part of that found by Smyth. Such was the state of our experimental knowledge of the subject until the time of the observations of the present Earl of Rosse, which, as marking quite a new order of accuracy in lunar heat measurements, we reserve for a subsequent and more detailed discussion.

* American Journal of Science, II, p. 329 (1820).

† London Phil. Magazine, vi, p. 138 (1835).

‡ Comptes rendus, xxii, p. 541 (1846).

§ Report of the Teneriffe Astronomical Experiment, addressed to the Lords Commissioners of the Admiralty.

‖ Phil. Mag., IV Series, xxii (1861).

¶ Proc. Royal Society, vol. xvii (1869).

** Comptes rendus, lxix (1869).

The amount of heat received from the moon, and the dependent question as to the temperature of the lunar surface, are subjects of greater interest to us than might at first appear. They are even ones in which we may be said to have a material concern, for until we know the temperature which an airless planet* would attain in the sun's rays, we can have no accurate knowledge of the extent to which the atmosphere of our own planet contributes to its heat, nor of some of the most important conditions of our own existence. Those conditions are only lately becoming known, for it has hitherto been supposed that the temperature of the earth's surface was chiefly due directly to the radiation which it receives from the sun. It has been admitted, indeed, that the air acts to some extent in increasing the heat by hindering the radiation from the soil, but the manner and extent of this action have scarcely been, as it now appears, even surmised. Thus Sir John Herschel, distinguished as he is as a meteorologist as well as an observer, transferring to the moon conceptions drawn from the supposed state of things here, states that the temperature of the moon's surface in the lunar day must rise to 200° or 300° Fahr., and sink nearly as far below zero during the long lunar night, and the idea that the airless surface of the full moon must be intensely hot (in comparison with ordinary terrestrial temperatures) has since been generally accepted. Almost the only dissenting voice has been that of Mr. John Ericsson, who asserts that the lunar surface must, on the contrary, be intensely cold. In the present writer's opinion, the temperature supposed by Herschel, if it exist on the moon, will imply the presence of an atmosphere there; and if we can set aside the weight of traditional belief and preconceived impression the supposition that the surface of the moon (if absolutely airless) must be cold, even in full sunshine, is one which, however paradoxical it may appear, we are led to entertain by evidence at the command of everybody who can ascend high in our atmosphere. As we go up a mountain, we do not find the soil growing hotter, but colder, in the sunshine; and at great elevations, where the barometer is low, and we are partly approaching the conditions which must prevail on the moon (if airless), we find the surface covered with perpetual snow, even under the intenser solar blaze. The direct rays, indeed, are hotter, but the radiation from the soil is so far greater than below that, on the whole, with every upward step that diminishes the protection of our atmospheric envelope, the surface tends to grow colder. This is a matter of the most frequent observation. It is confirmed by the analogous experience of aeronauts, and it bears to my mind but one interpretation, that if we ascended still higher, until the air had been left altogether behind, we should find there regions of still intenser cold than any which we have experienced at the highest altitudes attainable by man. It has indeed been urged that the cold of high altitudes is largely due to the expansion of ascending air currents and to analogous causes, but our conclusions also rest on means which are independent of this hypothesis. In 1881 the expedition under the writer's charge to Mount Whitney, in the Sierra Nevadas, made many hundred actinometric observations at altitudes of from 3,000 to 15,000 feet upon the direct solar radiation, and its power to heat a thermometer bulb virtually removed from every disturbing influence, so that it was possible to estimate the result which would follow if the air between it and the sun were wholly withdrawn; the conclusion being that the temperature of the surface of the earth in full perpetual sunshine would, in the *entire* absence of its atmosphere, not rise much more than 48° C. over that of surrounding space. Now, the "temperature of space" must be conceded to be a vague and unsatisfactory term. To give a meaning to the expression we must ask what final temperature the earth's surface would attain were the sun's radiation and its own internal heat wholly cut off, and it were warmed only by radiations from other heavenly bodies, visible or invisible, or by the dynamic effects of meteorites, &c. Pouillet's conclusions are well known. The writer has reached much lower values, which he will not undertake to here state or explain. For the present purpose it is sufficient to say that, in his belief, the surface temperature of our planet, so far as it is due to direct solar radiation, would probably be such, that every liquid we know, and perhaps every gas, would exist only as a solid, though beneath the vertical rays of the sun.

It is hence almost wholly to our atmosphere and its capacity (by selective absorption) of storing the solar heat that, in the writer's view, we owe the high temperature which makes our exist-

* Certain discrepancies between observations on terrestial and lunar radiation suggest to us, however, the possibility of the existence of a minute lunar atmosphere, too small for recognition by the telescope.

ence on the earth's surface possible. But such conclusions are, it must be admitted, in contradiction not only to the statement already quoted from Sir John Herschel, but also to what has been regarded as direct experimental evidence as to the high temperature of the lunar surface obtained by the Earl of Rosse (that is, if we admit the moon to be absolutely airless, as Sir John Herschel assumes it; for it is not only possible, but even probable, that a gaseous envelope to the moon, too small to make its presence known to ordinary astronomical observation, would greatly raise the temperature of its surface, and the not impossible existence of such an envelope must be here borne in mind).

Lord Rosse's observations, in which, as we have already remarked, anything like quantitative measurement of the lunar heat has for the first time been attained, we shall proceed to examine in some detail.

ABSTRACT OF LORD ROSSE'S PAPERS ON THE RADIATION OF HEAT FROM THE MOON, WITH COMMENTS BY THE PRESENT WRITER.

[Proceedings Royal Society, XVI, p. 436 (1869).]

The object of the observations discussed in this paper is the determination of what proportions the lunar radiation contains of—

1. Heat coming from the interior of the moon, which will not vary with the phase.
2. Heat which falls from the sun on the moon's surface and is at once reflected regularly and irregularly.
3. Heat which, falling from the sun on the moon's surface, is absorbed, raises the temperature of the surface, and is afterward radiated as heat of low refrangibility.

The apparatus employed was a 3-foot reflecting telescope, with two small condensing mirrors and thermopiles. (A table of observations made at different phases of the moon is then given in the original paper.)

Assuming that the moon is a smooth sphere * without specular reflection, we may compute, from theory, the form of a curve, representing the amount of heat received from the moon as a function of her phase. This curve approximates to a sinuous form, having a greater curvature at the maximum or at full moon than at the minimum. The observations given in the table fall tolerably well on this curve, and therefore the increase and diminution of heat with the varying phase of the moon follow the same law as that of light. The heat classified under head (1) can have no existence in this case.

We may seek to determine the relative proportions of (2) and (3) present in the lunar radiation by experiments with thin plate-glass.

About 80 per cent. of the solar radiation probably passes through glass. From direct observations it was found that only 8 per cent. of the moon's rays was transmitted by the piece used in the experiment.

From this result, and the generally accepted value of the ratio of sunlight to moonlight, we may deduce the ratio of solar to lunar heat radiation. To do this Lord Rosse assumes† that all luminous rays are transmitted by glass and *all obscure rays are stopped.*

We have then (according to him)

Percentage of luminous rays in lunar radiation 8 per cent.
Percentage of luminous rays in solar radiation 80 per cent.
Ratio‡ of solar to lunar luminous rays = 800,000 : 1.
Ratio of total solar to total lunar radiation = 80,000 : 1.

The correctness of the value obtained for this ratio is confirmed by other considerations: in the first place, by direct measurement.§

The sun's rays were reduced by passing through a small aperture, and the deviation of the galvanometer connected with that previously found for full moon, by using the deviation produced by a vessel of hot water as a term of comparison. The ratio thus found was 89,819 : 1.

We may also find a value of this ratio from theoretical considerations. To do this he makes the following assumptions the basis of calculation:

* Zöllner has shown that the lunar surface reflects nearly like a flat disk, but the observations of Lord Rosse, given in the table, are subject to so great uncertainty, that they would fall equally well upon Zöllner's curve.

† This assumption is so far from the facts now known as to make the determination of the heat ratio depending upon it of little value, for it is now ascertained that in the solar-heat spectrum formed by a glass prism nearly two-thirds of the energy is represented by invisible rays.

‡ Lord Rosse assumes the largest known determination (800,000 to 1) as the most probable. There are, however, from eight to ten determinations by observers of repute, and all of them smaller; and if we take the mean of these, we have, as elsewhere shown, the approximate ratio 400,000 to 1. The agreement of the value 800,000 with the values obtained by the other methods given, on account of the falsity of the fundamental assumption of the latter, can only be regarded as a coincidence.

§ To the *experimental* determinations of the solar and lunar-heat ratio we make no objection, and they are probably as reliable as any others.

1. The quantity of heat leaving the moon at any instant may without much error be considered the same as that falling on it at that instant.

2. The absorptive power of our atmosphere is the same for lunar and solar heat.

3. As was assumed in a previous formula, the moon is a smooth sphere, not capable of reflecting heat regularly.

Deducing a formula for the amount of the moon's diffuse heat received by the earth and substituting in it the necessary values of the quantities entering, we find that the amount is $\frac{1}{79000}$ of that received from the sun; a result which agrees well with the previous values.

The value of the galvanometer deflections was obtained by comparison with a vessel of hot water, which subtended the same angle at the thermopile as the large mirror. It was then found (the radiating power of the moon being supposed equal to that of the lampblack surface and the earth's atmosphere not to influence the result) that a deviation of 90 for full moon (about the average effect) appears to indicate an alteration of temperature through 500° Fahr. In deducing this result* allowance has been made for the *imperfect* absorption of the sun's rays by the lunar surface.

These observations must be regarded as merely preliminary, and the results may be subject to revision when more accurate measurements are obtained.

[Proceedings Royal Society, XIX, page 9, (1870)].

In the preceding paper it was shown that a large portion of the total lunar radiation consists of rays of low refrangibility emitted by the heated surface of the moon, which in the time of a complete revolution passes through a range of probably more than 500° Fahr. of temperature.

The ratio of the intensity of solar heat to lunar heat, as deduced from the observations, agreed well with values given by independent determinations.

Since the last communication, more accurate measurements have been made, with results substantially the same as those reached in 1869.

The glass used was found to transmit 87 per cent. of the sun's rays, 12 per cent. of the radiation from the moon, and 1.6 per cent. of that from a body at 130° Fahr.

Assuming† then that 92 per cent. of the luminous rays in either moonlight or sunlight is transmitted by glass, and 1.6 *per cent. of the obscure rays transmitted*, and taking 82,600 (found by direct experiment) for the heat ratio of solar to lunar rays, the resulting value of the light-ratio is 678300: 1, which agreeing well with the accepted value, shows that the heat ratio 82600 : 1 is very nearly correct.

In these experiments the quantity measured by the thermopile was the difference between the radiation from the circle of sky containing the moon's disk and that from a circle of sky of equal diameter not containing the moon's disk; no information in reference to the absolute temperature of either the moon or the sky results from them. The apparent temperature of the sky was found by comparisons with the radiation from blackened vessels of hot water at different temperatures to be from 17° to 32° Fahr. If the temperature of space be really as low as has been supposed, this result seems to indicate considerable opacity of our atmosphere for heat-rays of low refrangibility.

The observations made to determine the dependence of the heating power of the moon on her altitude, and the law of extinction of her rays in our atmosphere, are not very satisfactory on account of changes in the sky, which present great obstacles to measurements of this character.

The curve deduced in the first paper is given together with the later observations. As far as can be judged from so few and imperfect experiments, the maximum of heat seems to be a little after full moon.

[Proceedings Royal Society, XXI, p. 241, (1873).]

In this paper is given the result of recent and more careful observations made for the purpose of determining the dependence of the moon's heating power upon her altitude, the curve obtained being nearly but not quite the same as that found by Professor Seidel for the light of the stars, and showing a greater extinction of light than heat. By employing the table thus deduced, and introducing a correction for effect of change of distance of the sun, a more accurate phase curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Professor Zöllner gives for the moon's light. At 79° zenith distance, the effect of atmospheric absorption is about one-tenth of the whole amount. Lord Rosse observes that this difference may be due to the fact that Seidel's light observations were made on the stars, not on the moon, and that it hence does not necessarily imply a different law for the extinction of light from that for heat.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, it was found that the heat and light diminish nearly, if not quite proportionally, the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half what they were before and after contact with the penumbra.

The probable error of a single set of 10 galvanometer readings at this time is given as about 19 per cent., or about 8.7 per cent. for the probable error of a night's observation; but Lord Rosse concludes that so large constant errors were probably present, that any increase in the number of sets was almost powerless to obtain a more reliable result.

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* It will be seen by reference to a subsequent paper that this result is considered to be erroneous. What is meant by an allowance for imperfect absorption by the lunar surface is not clear.

†The assumptions we have italicized are little nearer the truth than those of the previous paper and the result entitled to little more weight.

[Nature, XVI, p. 438 (1877). Letter by Lord Rosse, replying to M. Raillard, who attributes the reddish tinge of the totally-eclipsed moon to self-luminosity due to the high temperature acquired under the sun's rays, and cites observations of Lord Rosse in support of his views.]

M. Raillard is mistaken in supposing that Lord Rosse estimated the temperature of the lunar surface at 500° Fahr. This was the range of temperature which a lamp-blackened vessel must have in order to exhibit effects similar to those of the moon in its different phases, deduced from early observations. More accurate observations (described in previous papers) show that this range is much more nearly 100° C., a large error having crept into the previous work. The observations made during the total lunar eclipse show that the diminution of heat kept pace with that of light. Probably not more than 5 per cent. of the heat acquired since new moon is retained till the middle of a total eclipse, although it has been shown that this heat has been absorbed by the lunar surface and reradiated. We must therefore fall back upon the usual explanation for the reddish color of the moon's surface during a total eclipse.

Urania I (1881).

On comparing the heat curves between new and full moon with that between full and new moon there appeared no conclusive evidence that the lunar surface required time to acquire the temperature due to the radiation falling on it. Accordingly, observations were made at the eclipses of November 14, 1872, and August 23, 1879, but the result of these was to show that the decline and subsequent increase of the heat took place as rapidly as that of the light.

[Nature, XXX, p. 589 (October 16, 1884). Description of observations made during the total lunar eclipse of October 4, 1884, by Otto Boeddicker, at the Earl of Rosse's observatory.]

The apparatus used was the same as that already described. Clouds prevented observations until 30 minutes before the beginning of the total phase, when the sky became exceptionally clear.

Two hundred and eleven readings of the galvanometer were taken, the time of exposure being 1 minute for each. (A curve representing these observations is given.) No observations were made during the total phase on account of the difficulty of judging when the image fell on the thermopile, but near the beginning and end of totality the effect was masked by the irregularities of the galvanometer, and was smaller than the probable error of observation. The minimum of heat seems to be *later* than that of light. As the moon emerged from the earth's shadow, so slowly did the readings of the galvanometer increase again that, about twenty minutes after the total phase was over, the almost entire absence of any effect led the observer to think that the small condensing mirrors must be covered with dew, which however was not the case. *

PRELIMINARY OBSERVATIONS AT ALLEGHENY.

OBSERVATIONS ON LUNAR HEAT.

The first measures of lunar heat at Allegheny were made on the evening of November 12, 1880,† more with a view to testing the sensitiveness of the then recently invented bolometer than for the sake of the measures themselves. The lunar rays were concentrated upon the face of the bolometer by means of the 13-inch equatorial of the observatory and a smaller convex lens near its focus, and an average deflection of 42 divisions of the galvanometer scale was obtained. The exposures were made by directing the telescope upon the moon after the galvanometer needle had come to rest, while the telescope was pointed at the neighboring sky.

On June 21, 1883, the bolometer and its adjuncts having been much improved in the interval, measurements of the lunar heat were resumed with apparatus better adapted to the purpose. The thick glass lenses, used in the previous experiments, absorb and reflect a large proportion of the already sufficiently minute amount of heat at disposal, and the rays absorbed are those whose presence or absence chiefly affects the conclusions to be drawn from the results of the observations. In the new experiments, therefore, the dioptric system for condensing the lunar rays was replaced by two silvered-glass mirrors, the absorption of silver for every heat ray of the spectrum having been determined by previous experiments here. This absorption, as is well known, affects chiefly the blue and violet rays, in which but a small proportion of the total energy resides. Our own observations show this absorption (by silver) to be very small and nearly constant throughout the infra-red. The sensitiveness of the bolometer, and its accuracy, enable us, as will be seen, to

*Although over one fourth of the moon's surface must have emerged from shadow at this time, it must be remembered that it was still covered by the earth's penumbra, so that the small heating effect is less surprising. If the moon parts with its acquired heat so soon as these eclipse observations seem to indicate, it is difficult to see how the maximum of heat could occur at an appreciable time after full moon. It is in any case hard to admit that this heat from the lunar surface, which the moon has been absorbing during many days of continuous sunshine, is parted with at once, the whole earthward surface of the planet cooling almost instantaneously.

†American Journal of Science, CXXI (March, 1881).

obtain with a comparatively small mirror, concentrating much less heat than that dealt with by the 3-foot reflector of Parsonstown, much more consistent indications.

The lunar rays, reflected from the 12-inch silvered mirror of a large siderostat, pass horizontally through an 8-inch circular aperture in the north wall of the observatory dark chamber, and fall upon a 10-inch concave silvered glass mirror of about 30 inches focus, mounted on a very solid tripod stand.* The bolometer, in a case specially designed for this work, is mounted on a sliding carriage directed toward the center of the mirror, so that it can be adjusted to such a distance as to bring the working face of the instrument into the plane of the lunar image. The bolometer case referred to has a series of circular diaphragms of various apertures so disposed as to protect the bolometer itself both from air currents and extraneous radiations, while just admitting the cone of rays from the concave mirror.

This mirror is inclined slightly to the incident rays, so that the bolometer can be placed a little to one side of the large aperture in the wall and not obstruct them. The lunar image can then be adjusted by means of the milled-headed screws, by which the mirror is secured to the vertical plate, so as to fall truly on the bolometer strips before observation, and afterward in actual observation be carried on or off by a motion of the siderostat mirror outside, or by the moon's own motion in the heavens. In either case no screen is interposed, and no alteration in the relation of the radiating objects around takes place in reference to the bolometer, which experiences no changes, except those which come from its alternate exposure to the moon and the neighboring sky.

The lunar image is about 0.26 inch in diameter, and when properly directed is received by the working face of the bolometer, which it very nearly covers; hence it will be seen that (neglecting the absorption of the mirror, which is very small for invisible heat rays), remembering that only eight inches of the mirror's diameter is utilized, the intensity of the lunar heat was increased from 780 to 1,050 times, according as the distance of the moon from the earth varied. On a clear night with a full moon, and the galvanometer in its condition of then greatest sensitiveness, a deflection of 300-millimeter divisions of its scale could be obtained (in 1883), but toward the close of 1884, with still further improvements in the apparatus, this limit was much exceeded.

The "exposures" in later and adopted measures were only made, as we have said, by moving the image of the moon on and off the strips of the bolometer, by slightly inclining the siderostat mirror, thus simply replacing the image of the lunar surface by one of the adjacent sky. This was readily effected by means of a pulley on the azimuth screw of the siderostat from which a cord led into the building.

This method, we repeat, leaves the radiation from the apparatus itself unchanged by the introduction of the lunar heat, and avoids the disturbing influences which come from the interposition and withdrawal of a screen. Other methods, however, were tried in these earlier experiments, such as that of displacing the image by turning one of the screws of the concave mirror mount. This method, though sometimes yielding identical results, was found to be liable to errors, as was also that of the use of a screen, and in a still greater degree. It is not always easy to point out the exact nature of the error introduced in these delicate determinations, but we will give some of these preliminary observations to show the nature of the discrepancies presumably due to such methods of exposure.†

The character of the lunar energy, as compared to the solar, was first investigated as in Lord Rosse's experiments, by determining the relative transmissibility of the lunar and solar rays, as a whole, by certain pieces of glass, which were interposed in the path of the rays immediately in front of the bolometer case. Four pieces of glass were used for this purpose. The first was a disk 4.2 millimeters thick, of the same glass as the prism made by Adam Hilger, of London, used in a previous determination of wave-lengths in the infra-red of the solar spectrum; the second was

* See Plate 1, where M is the concave mirror; B, the bolometer; C, the cable connecting it with the galvanometer; G, the place at which the glass is interposed.

†The apparatus above described, as employed in 1883 and the summer of 1884, is substantially the same as that used in the later lunar heat measures, the chief improvements, since it was completed, having been made in the galvanometer and other electrical adjuncts of the bolometer. For a description of this instrument the reader is referred to earlier papers by the writer in the Proceedings of the American Academy of Arts and Sciences, xvi, 1881, and the American Journal of Science for March, 1881, and to details given later on in the present memoir.

a piece of plate glass ("A") 6.9 millimeters thick, apparently of English make; the third ("B") a piece of American plate glass, slightly greenish in hue, 6.6 millimeters thick, and the fourth a large pane of American window glass of good quality.

On July 9, 1883, October 4, 1884, and November 26, 1884, the diathermancy of these specimens of glass was determined for solar rays. The values obtained were:

	Per cent.
For the Hilger glass	86
For Glass A	86
For Glass B	77
For the large pane	76

the zenith distance of the sun being about 50°, or in the last case over 60°.

We have not as yet reduced these observations taken at somewhat different altitudes of the sun and moon to one common altitude, because it is sufficiently obvious from a comparison of the above figures without such a reduction with those indicating the absorption of the lunar heat by the same specimens of glass, that these absorptions are strikingly different in the two cases, and far more so than any difference in the altitudes of the bodies under consideration can account for. We will pass, then, to our preliminary observations on this unequal absorption before introducing small corrections which would be superfluous at this stage of the inquiry.

Preliminary measurements were made in pursuance of this system on the night of June 21, 1883, when all atmospheric conditions appeared to be favorable. A screen was, however, employed on this night to cut off the lunar rays, and the exposures made by withdrawing it (a method not capable of giving exact results).

If the glass be interposed while the lunar rays are falling on the bolometer, radiations from or to its substance will in general be confused with the effect to be studied, and further the radiations of low refrangibility from surrounding objects, such as those from the substance of the condensing mirror, will be cut off by it. It is always a condition of good observation, then, that the glass be placed in front of the bolometer and the instrument allowed to register its separate effect before the lunar rays are allowed to fall upon it; and this method has always been used.

It was found from the series of observations that the percentage of the total lunar heat transmitted by glass A was 70 per cent. and from a second series 54 per cent., giving a mean for glass A of 62 per cent.

For the percentage transmitted by glass B were obtained the values 60 per cent., 46 per cent., and 58 per cent., giving a mean of 55 per cent.

The discrepancies in these preliminary results were partly due to the above-mentioned erroneous method of exposure by withdrawal of a screen, but also to the difficulty of telling when the lunar image was exactly coincident with the bolometer face, since, when this was not the case, part of the heat was wasted and the deflection obtained too small, and the eye could not be safely brought into a position where the strips could be seen, since then the radiation from the observer's face gave a large deflection. This difficulty was subsequently overcome by placing a large sheet of glass behind the mirror, through which the observer could regard the bolometer strips without producing any disturbance of the galvanometer, the radiations from his face being completely intercepted by the glass. The conclusion, apparently resulting from the preliminary experiments where a screen was used, is that the specimens of glass appeared to transmit the greater part of the moon's rays; but that the apparent transmission of the glass should be greater when exposure is made by the withdrawal of a screen may be inferred from the following considerations. The screen is in general warmer than the external air, and if withdrawn while the bolometer was directed to the sky near the moon, a negative deflection of the galvanometer would be produced. When the screen is withdrawn while the bolometer is directed upon the moon, the heating effect of the latter is partly counteracted by the cooling effect of the sky or air between us and the moon, and the deflection obtained is smaller than that which would have been produced if the bolometer had been continuously exposed to the radiation from the sky. When, however, the glass is interposed, it forms a barrier to the interchange between outside objects of low temperature and the bolometer, and nearly the same deflection is obtained whether a screen is used or not. By the use of a screen, therefore, the apparent transmission of lunar heat by glass is larger than it otherwise would be.*

These and other preliminary observations, then, are not used in the final results, but it has been thought worth while to refer to them to indicate some of the subtle causes of error which beset the commencement of such a research.

SUBSEQUENT OBSERVATIONS ON THE TRANSMISSIBILITY OF GLASS FOR LUNAR RAYS.

In October, 1884, these observations were again taken up and the transmissibility of the same pieces of glass redetermined.

The method of procedure was as follows: The apparatus being in adjustment and the galvanometer needle in a position of equilibrium, the lunar image was thrown on the bolometer by turning one of the concave-mirror screws, and the deflection of the galvanometer noted. Then the lunar image was thrown off the bolometer, and the new position of equilibrium noted, to which the galvanometer needle returned. This was generally slightly different from the original position. From these readings was obtained the effect of the uninterrupted lunar beam. A piece of glass was then interposed immediately in front of the bolometer case, and after the galvanometer needle had taken up a new position of equilibrium, caused by the alteration of conditions in regard to its thermal exchanges with the glass itself and with outside objects, the same operation was repeated, and the effect of the lunar ray obtained after it had suffered reflection at the surfaces and absorption in the substance of the glass. The following results, which are the means of repeated observations, were obtained by the writer under favorable conditions, except that exposures were made by touching the adjusting screw of the concave mirror, instead of that of the siderostat mirror. The image of the moon is thus replaced by that of the neighboring sky, but since the concave mirror is within the building, an alteration of the thermal conditions may be produced by an increased reflection of heat from the walls of the apartment in the mirror.

* To put this into symbolical form, let C = the amount of heat received from the walls of the bolometer case, s = the heat received from the screen, g = the heat received from the substance of the glass, S = the heat received from the sky, m_1 = the portion of lunar heat transmitted by glass, and m_2 = the portion of lunar heat absorbed by glass.

First step of observation.—The bolometer is exposed to the radiation from the screen, which is interposed between it and the sky. The heat received is then $C + s$.

Second step of observation.—The screen is then withdrawn and the bolometer exposed to the moon. The heat received is $C + m_1 + m_2 + S$.

The deflection of the galvanometer which is produced is due to the difference between these two amounts of heat or to $m_1 + m_2 + S - s$.

Third step of observation.—The screen is again interposed, and the plate of glass, completely cutting off the radiation from the screen, is placed in front of the bolometer, which then receives the amount of heat $C + g$.

Fourth step of observation.—The screen is now withdrawn and the bolometer again directed toward the moon. The heat from the sky, S, which consists entirely of radiations of long-wave length, is also completely cut off by the glass, and the amount of heat received by the bolometer is $C + m_2 + g$, so that the resulting deflection of the galvanometer is proportional to $(C + m_2 + g) - (C + g) = m_2$.

The ratio of the two deflections obtained as above is the apparent transmissibility of glass for lunar heat, and it is therefore

$$\frac{m_2}{m_1 + m_2 + S - s} = t_1$$

If observations had been made without the use of a screen, by moving the siderostat mirror, the transmissibility would have been

$$\frac{m_2}{m_1 + m_2} = t_2$$

The quantity of heat, S, depends upon the apparent temperature of the sky; that is, upon the temperature of the external air, and as this, except in an unusual combination of circumstances, is lower than the temperature inside the building, $S - s$ is negative, and consequently $t_1 > t_2$.

Measurements.

Radiation measured.	Deflection.	Amount transmitted.
Direct lunar heat	260	
With Hilger glass interposed	70	.29
Direct heat	230	
Hilger glass	69	.30
Direct heat	234	
Glass B	77	.33
Direct heat	237	
Glass B	64	.27
Direct heat	233	
Glass A	71	.30
Direct heat	238	
Glass A	62	.26
Direct heat	236	

From these measurements it was concluded that the Hilger glass transmitted 29 per cent., the glass A 28 per cent., and the glass B 30 per cent. of the lunar radiation under the conditions in question.

A similar series of excellently accordant observations on the same evening by another observer placed the transmissibility of the Hilger glass at 27 per cent., of glass A at 27 per cent., and of glass B at 26 per cent.

The most striking feature about these results is their very fair agreement among themselves and yet their discordance with the previous measures of June 21, which also exhibit no striking discrepancies of an order equal to that existing between the two sets of measurements. It was therefore concluded that this discrepancy was in all probability chiefly due to purely local causes affecting the condition of the apparatus at the time of experiment, which we have reason to believe is in a large measure accounted for by the differing methods of exposure, and at the following lunation the measures were repeated, varying these conditions with especial regard to the following points: (1) Temperature of different portions of the apparatus, particularly of the large concave mirror; (2) temperature of the glass; and (3) place at which the glass was interposed. For the purpose of varying the latter condition, the large pane of window glass, already referred to, was fixed in a frame so fastened to the large flat of the siderostat that the lunar rays incident on the flat were first obliged to pass normally through the glass, without, however, being intercepted by it on their way to the aperture in the wall of the building.* The glass could be instantly withdrawn from the frame when desired and interposed immediately in front of the bolometer as in previous experiments, or elsewhere in the path of the rays. These later experiments were carried out with all due precautions, exposures being made only by inclining the siderostat mirror by means of its azimuth motion in the manner previously described. The transmission of the large pane for solar rays was determined with special care by a long series of observations, and was substantially the same as that given by former measures. The same apparent transmissibility was found for all positions of the glass, whether in the open air above the siderostat mirror or immediately in front of the bolometer case inside the building.

Experiments on the effect of varying the temperature of the glass and concave mirror by warming were only partially successful, since the immediate effect in either case was naturally that the progressive cooling of the heated object produced a violent "drift" of the galvanometer needle, so that measurements could only be resumed when the temperature had fallen nearly to that of the surrounding objects. Within this limited range of temperature, however, the transmission of the glass did not appear to vary as it presumably would have done had there been any change in the hygrometric condition of the surface of the plates or other disturbing cause.

* Since glass is athermanous to radiations from sources of such temperature as the bolometer strips or the walls of the room, its position with reference to these might (conceivably) affect the result. Although the mode of observation was calculated to eliminate any such effect, the experiment of placing the glass outside the building was therefore tried.

For the transmission of the large pane for lunar radiation were obtained the values:

	Per cent.
Mean of observation of November 26	12.2
Mean of observation of December 2	14.5
Mean of observation of December 3	14.9
Mean of all	13.9

(the moon's zenith distance being about 45°.)

Observations on the variation of the coefficient of transmissibility of the lunar rays at different altitudes of the moon have been made at every opportunity, but the results are so dependent on fluctuations in our atmospheric conditions that they are at present only to be interpreted as showing that, if there be a difference in transmissibility of glass for lunar rays at different altitudes of the moon, this difference is not a conspicuous one.

Among the substances, whose power of transmitting the lunar radiation was tested, was a very thin disk of polished ebonite (thickness = 0.28 millimeters) through which the moon, when viewed with the naked eye, appeared of a dark-red color. It was found by experiment with the bolometer that this ebonite disk transmitted 6.9 per cent. of the moon's rays. Its transmission of the solar rays was 32.4 per cent. The transmission of the large pane for solar rays was also carefully redetermined, with the following results:

	Per cent.
Mean of observation of November 26	75.6
Mean of observation of December 3	75.1

These values are quite in accordance with those previously given.

All of the later and more careful observations show, therefore, that, whereas nearly 76 per cent. of the total apparent solar radiation is transmitted by the large pane of glass, only about 14 per cent. of the total apparent lunar radiation is transmitted.

PRELIMINARY PHOTOMETRIC OBSERVATIONS IN 1883.

The low transmissibility by glass which the lunar rays have been shown to possess by the experiments described in the first part of this paper is quite confirmatory of the experimental results of Lord Rosse, though not necessarily of his inferences from them. As we shall see, it may be partly accounted for by the supposition that the rays which reach us have suffered selective reflection at the surface of the moon. It is quite evident that, if selective absorption of heat take place, we ought to see it in the study of those heat rays which are also seen as "light." Moreover, as rays emitted from a source even of the temperature of boiling water can have nothing to do with vision, we shall not be liable to confound what we *see* with any effect due to radiation from the lunar soil, for what we thus observe must be due to reflected heat only (since "light" and "heat" are but names given to different manifestations of the same energy). Accordingly, if photo-spectrometric observations on homogeneous rays show a progressive selective reflection such that rays of low wave-length (such as are more absorbable by glass) are present in greater proportion after reflection from the moon than before, we shall undoubtedly be justified in concluding that the effect observed by Lord Rosse is in part, at any rate, due to this cause, and not *necessarily* to the presence of heat of low refrangibility radiated from the lunar soil. From the fact that the lunar light is not white like the sun's, but yellowish (Sir J. Herschel compares the moon's surface to that of sandstone rock), it was antecedently probable that such was the case. The fact has indeed been independently determined, but the writer was not familiar at this time with the work of others in this direction. The following apparatus, which was fitted up in June, 1883, was employed in the months of June, July, and October of that year for photometric comparisons of moon and sun light. It is not described more minutely because all the observations were afterward repeated with an improved form of it hereafter described.

The lunar beam, reflected from the siderostat mirror, passed into the dark room, fell on a silver-on-glass mirror of seven inches aperture and five feet focus which formed a lunar image on the lower half of a slit, whence the light passed through a collimating lens, and fell upon a large Rutherfurd grating of 17,296 lines to the inch, whose diffracted rays were viewed by an observing telescope. The inclination of the grating was determined by a graduated circle and vernier, so that by use of the customary formula the exact wave-length of the color or line in the center of the field could be computed. On the upper part of the slit was a prism of total reflection which brought in the rays from an Argand burner arranged to slide at right angles to the axis of the collimating telescope along a graduated scale. The amount of gas supplied to the burner was controlled by a meter. Accordingly, a spectrum from a flame of standard and constant brightness was formed by the same grating in juxtaposition to the lunar spectrum immediately under it in the apparent field and viewed by the same eye-piece. The lamp was now withdrawn or approached until some particular wave-length (*e. g.*, the yellow about $0^{\mu}.6$) was judged to be of like strength in either spectrum. Under these conditions if the grating was rotated so as to bring in more of the blue end of both spectra, the moonlight spectrum grew constantly brighter relative to that of the gas-light, so that it was necessary to strengthen the latter light to re-establish equality. The field was limited by a diaphragm to a narrow strip of both spectra, whose edges were brought as closely into juxtaposition as possible, and numerous series of comparisons were taken throughout the visible spectrum, which after the requisite corrections and reductions gave the relative intensity of the lunar spectrum in each part to that of the gas. The same apparatus was used for the solar comparison in the same way, except that the stronger sunlight was allowed to enter through a smaller aperture and was diffused, instead of concentrated, by being allowed to fall on a convex silvered mirror. It was evident that the proportion of blue in the sunlight was greater than in the moonlight, as the following results show.

Observations of June 20th to 22d.

(Corrections for altitude have not been applied.)

For wave-length μ .474, sunlight 2,483,000 times moonlight.
For wave-length .581, sunlight 232,140 times moonlight.
For wave-length .625, sunlight 30,600 times moonlight.

These comparatively rough preliminary values are not believed to have any great quantitative accuracy, but they at least show clearly that there is selective absorption of light (and hence of heat) throughout the visible lunar spectrum, of such a kind that the rays less transmissible by glass will be found (so far as our investigation extends) in greater proportion in moon heat than in sun heat, irrespective of any question as to sensible radiation from the lunar soil. It was evident that the photometric method was liable to error considerable enough to make very considerable discrepancies between the work of careful observers, and the general results only are given above, because the work of 1883 was supplemented by a more careful series of observations in 1884, which we now proceed to give in detail.

GENERAL CONSIDERATIONS.

Zöllner has shown that, owing to the irregularities of its surface, the full moon does not reflect as a smooth sphere would do, but very nearly as a flat disk of like reflecting power, and filling the same angle. Such a disk, if it presented, as seen from the earth, the mean semi-diameter of 15′ 35″, and if it diffused all the solar energy which fell on it,* would send to us $\frac{1}{97300}$ of what the sun does, which is the portion of the solar energy which we should receive from such a moon, reflecting perfectly (not specularly, but in all directions) all the solar energy which fell on it. The moon, however, is far from being a perfect reflector. The color of its surface is comparable (as we recall) to that of sandstone rock, and hence it must reflect selectively, and, as far as we can see, in such a manner that the longer wave-lengths are in larger proportion in the reflected than in the original solar beam, in which. roughly speaking, the luminous energy is about one half of the non-luminous or dark heat. Since the moon then only imperfectly re-

* Considerable difference may exist even in values obtained from such geometrical considerations. Thus Lambert's formula gives the number $\frac{1}{73000}$, which is nearly that used by Lord Rosse; and George P. Bond (Memoirs of American Academy, vol. viii) uses the value $\frac{1}{45000}$, where we have taken $\frac{1}{97300}$.

flects or diffuses, part of the solar energy must be absorbed and re-radiated as dark heat. We make no doubt, then, that the lunar soil radiates heat toward space. The real questions at issue are "At what temperature does it so radiate?" "Can we have any experimental knowledge of such dark heat radiation at the earth's surface?" If we suppose, for instance, the lunar soil to be heated by the sun 50° C. above the temperature of surrounding space, then in the case of this very considerable supposed heating effect, the moon's surface will remain far below zero in the sunshine, and though it may be said in one sense to radiate heat to the earth, yet since it is in this case below the mean temperature of the earth's surface, we should obtain no sensible heat from it, even were our atmosphere altogether absent, while the actual presence of our atmosphere, athermanous, as it is generally believed to be to such radiations, would render their determination hopeless. Whether the moon be a perfectly diffusive body or the actually imperfectly diffusive one, we get the same amount of heat from it; for it will finally attain a condition of heat equilibrium in which it will send away as much as it receives. In the first hypothesis, what it sends away will be purely reflected or diffused energy, of wave-length corresponding to what it has received from the sun; in the second hypothesis, the radiant energy will be partly reflected, and partly that of much lower wave-length emitted by the soil. The second hypothesis, doubtless is the true one; but the question before us is, "Is this re-radiated heat sensible?"

From the fact that the lunar energy appears less transmissible by glass than the solar it has been assumed that the entire effect is due necessarily to a large heat radiation from the lunar soil, which our atmosphere transmits and the glass stops. Before we accept this hypothesis we must repeat that it does not necessarily imply this, for we have only to suppose the selective reflection exercised on the solar rays at the surface of the moon to be such as to send us in the reflected rays an undue proportion of those which glass absorbs, to account, at least in part, for the observed effect. We will pass, therefore, to a series of observations which show more clearly than any yet given that a selective absorption of such a character does actually take place.

PHOTOMETRIC OBSERVATIONS IN 1884.

Comparative photometric measures of the intensities of solar and lunar rays are of importance, as we have seen, to our heat determinations, and especially is this the case when such measures are combined with others (to be shortly given) of the comparative amounts of heat received from the sun and moon. The complete knowledge desirable would tell us of the special ratio of each separate heat or light ray, but even a knowledge of the ratio of the total sunlight to moonlight and the total sunheat to moonheat will be valuable. If, for instance, it were found by purely optical means that the intensity of sunlight was m times that of moonlight, and by an instrument like the thermopile or bolometer, in which the registered effect of the radiation is proportional to the amount of energy which resides in it, that the heat received from the sun was only n times that from the moon, even such a result would enable us to draw some inference as to the general character of the lunar energy, and hence of the conditions of temperature of the moon's surface. For, in the case above stated (supposing $m>n$), the given relation between the light and heat ratios could be explained only on the supposition that the energy was distributed differently in the two spectra, a larger portion of that residing in the lunar rays being unable to produce any physiological effect when received upon the retina or incapable of being interpreted as light, and hence that the surface of the moon had either selectively reflected the solar rays or had added to them radiations from its own substance indicative of a considerable individual temperature. We have seen, however, that a difference in the direction of the above supposition is to be expected from the effects of selective reflection at the moon's surface.

The chief objection to such a comparison between the light and heat ratios of the sun and moon is the difficulty of making the necessary measurements with the requisite degree of accuracy; so that, unless the difference were extreme, it would be masked by the effects of the errors of observation. The photometric comparisons are generally made with the aid of an artificial source of light of intermediate brightness, which at once introduces a considerable degree of uncertainty into the problem on account of its variations in intensity. Differences in altitude and changes in the state of the atmosphere have also great influence upon the result; and it has been shown by the writer how great is the difficulty of making certain allowance for the effect of these unequal circumstances by processes of mathematical computation. Even for the relative total brightness of the sun and moon very discrepant results have been reached, which may be best exhibited in the form of a table of the principal determinations.

300, 000 : 1	(Lambert.)
400, 000 : 1	(Lambert, allowance made for various errors.)
300, 000 : 1	(Bouguet, Essai d'optique, &c.)
801, 000 : 1	(Wollaston, Phil. Trans. (1829), vol. 8.)
480, 000 : 1	(Bond, Memoirs American Academy, vol. 9.)
618, 000 : 1	(Zöllner, Photometrische Untersuchungen, p. 105.)
350, 000 : 1	(W. H. Pickering, Pr. Amer. Academy, 1880.)
70, 000 : 1	(Sir William Thomson.)

There has been no essential improvement in such photometric processes as are here in question since the early measures by Bouguer and Lambert. Zöllner's are perhaps made with more care than others, but giving all these values equal weights, we have 405,800 : 1 as the mean ratio. It is sufficiently evident that the limits of error are here wide, and we shall adopt 400,000 to 1 as the most probable value.

Recurring now to the comparison of separate spectral rays in sunlight and moonlight, we find that investigations have been independently made by two competent observers.

In those of W. H. Pickering,* in which light from various sources was compared with that from a standard Argand gas-burner at four different parts of the spectrum, there is a very great preponderance of violet in the solar rays as compared to the lunar. It is possible that the difference is too great, but we have already remarked upon the extreme difficulty of real accuracy in such determinations, and our own earlier observations are of a like order of discrepancy.

Dr. H. C. Vogel,† compared, by means of a spectro-photometer, in which a petroleum lamp served as a standard, moonlight and sunlight which had been reflected from various kinds of rock. As a result he found that a selective absorption of the more refrangible rays of the spectrum took place on reflection of the solar rays by the surface of the moon, although not sufficiently pronounced to indicate any very decided color in the substance of which it is composed. The moonlight agreed best with sunlight reflected from yellowish gray sandstone.

The spectro-photometer used at Allegheny in the later observations (in 1884)‡ to determine the amount of the selective reflection under consideration was the result of the experience obtained in 1883, and at the same time not dissimilar in principle to those employed in the researches of Vogel and Pickering, the brilliancy of the two spectra being compared at different points by means of an artificial source of light of supposed constant intensity. This artificial source was a kerosene lamp in which the oil was kept at a constant level, with Argand burner, and screens so placed before the glass chimney as to limit the effective part of the flame to a cylindrical portion 10 millimeters high, taken where it was brightest. The lamp was trimmed and cleaned before each set of observations, and although the constancy of its light seemed to be all that was desired, the quality was so different from that of either of the two heavenly bodies to be compared that the accuracy of the observations was not so great as would have been obtained if a source like the electric light, for example, had been used.

In order to carry out the measurements, the intensity of the sunlight had to be diminished, and that of the moonlight increased until they were both comparable with the standard. In doing this no attempt was made to determine the amount of diminution or increase, although a rough approximation to this is possible, but as only relative brilliancies in different parts of the two spectra were desired, attention was mainly paid to securing a convenient intensity in the light to be compared.

Plate 2 represents the arrangement of the apparatus. The light, reflected horizontally by the 12-inch silvered mirror of the siderostat, enters the dark room by an aperture, A, in the north wall. Here, if it is sunlight which is being compared, all is stopped except what passes through a small cir-

* Proc. Amer. Academy, 1880, p. 236.

† Monatsberichte d. Konigl. Akademie d. Wissenschaft z. Berlin, Oct. 21, 1880.

‡ These observations were conducted by Mr. J. E. Keeler of this observatory.

cular aperture 4.86 millimeters in diameter, in the center of a cap covering the object-glass of a small telescope, *D*, of about 520 millimeters focus. On leaving the eye-piece of this telescope the sunlight is spread out into a diverging cone of rays, which at the distance of the photometer slit *S*, 2610 millimeters beyond the eye-piece, has a diameter of 652 millimeters. Its intensity has therefore been weakened about 18,200 times (independently of the absorption of the glass, which is not a a factor in our qualitative determination).

S is the slit of a grating spectroscope. The collimator *C* has a focal length of 1254 millimeters and an aperture of 57 millimeters. The observing telescope *T* is much shorter, having a focal length of but 400 millimeters (in order that the head of the observer may not interfere), and is set at a fixed angle of about 49° to the collimator. A holder within the case at *G* carries a flat Rowland grating with a ruled surface 51.6 by 35.0 millimeters, and with the number of lines per millimeter equal to 568.4. This grating, which gives very brilliant and very perfect spectra, was used in such a position that the normal to its surface fell between the two telescopes, the comparisons being made in the brighter first spectrum. Its angular position is indicated by a divided circle and vernier, reading to minutes on the outside of the case.

The lower part of the photometer slit is covered by a totally reflecting prism, *P*, which cuts off the sunlight entering there and substitutes for it the light from the standard lamp, *L*. Two spectra in close juxtaposition are therefore seen in the eye-piece of *T*, the upper belonging to the lamp and the lower to the sun. By means of a 2-millimeter blackened cardboard slit in the common focus of the object-glass and eye-piece, the range of wave-lengths included in the field of view was limited to $0^{\mu}.0048$, or about eight times the interval between the *D* lines.

The lamp has already been partially described. It was fastened to a slider, which could be drawn by the observer to and fro along a graduated scale, at right angles to the collimator so as to approach or recede from the slit, by pulling a cord. A heavy screen which hung down to the level of the photometer scale concealed the lamp from the observer, who was thus unaware of its position while making a measurement (except from the appearance of its spectrum in the eye-piece of the telescope) until the index had been read, and thus any bias resulting from a preconceived opinion as to the proper position of the lamp was avoided. The range of the scale was 20 decimeters, and its zero-point was so adjusted that the reading of the index of the lamp-carriage was the distance of the center of the flame from the slit of the photometer. On account of the great difference in the quality of the lights compared, the range of the scale proved to be insufficient, and the wheel photometer, an instrument presently to be described, was used to diminish the more intense light by a given ratio.

When moonlight, instead of sunlight, was compared with the standard, the diminishing telescope was removed, and a telescope of 1,054 millimeters focus and 77 millimeters aperture, with the eye-piece removed, was placed on the axis of the beam from the siderostat, so as to form an image of the moon on the upper half of the photometer slit.

The wheel-photometer, referred to above, consists of two circular disks of sheet-zinc about twenty inches in diameter, each pierced near the circumference by eighteen radial apertures separated by spaces of the same width. The two disks may be rotated past each other with considerable friction, enough to hold them firmly in relative position, and are held by an axis passing through their centers, by means of which, and a multiplying wheel connected with it, they may be rotated in a vertical plane with great velocity as a single wheel. If they are adjusted to coincide, and rotated in front of a source of light, they diminish its brilliancy one-half, although, on account of the persistency of vision, the eye does not perceive any flickering or unsteadiness caused by the interruptions of the spokes. A graduated arc is attached to one of the disks, and an index to the other, so that the apertures may be adjusted to any width from the full opening down to zero. Thus the intensity of a luminous source may be diminished to any fraction less than one-half of its original value.

On looking into the eye-piece of this apparatus (Plate 2) two nearly square patches of light were seen, the lower belonging to the sun or moon, and the upper to the lamp. The color of the light would depend, of course, upon the position of the grating. The observations were made by sliding the lamp along the scale, by means of its cord, until these two squares of light were of equal intensity. Then, if the intensity of the standard is known for all points of the scale, we obtain the intensity of the sunlight or moonlight at that part of the spectrum. If the wheel-photometer was used, a proper factor must be introduced to give the degree of diminution caused by it. Eight points in the spectrum at which comparisons were to be made, which we may roughly designate by their approximate colors, were selected. Their wave lengths, and the settings of the grating circle, together with those for several of the Frauenhofer lines, are given in the annexed table.

Point.	Color.	Line.	Setting.	Wave-length.
			° ′	μ
1	Deep red.......	*B*...	84 5	0.687
....	....	*C*...	84 35	0.656
2	Bright red.....	....	84 47	0.649
3	Orange........	....	85 40	0.599
....	....	*D*...	85 49	0.589
4	Yellow	....	85 52	0.586
5	Green	(*b*)..	87 8	0.518
6	Blue	*F*...	87 42	0.486
7	Bright violet ..	....	88 0	0.470
8	Deep violet....	....	89 0	0.415

A table giving the intensity of the illumination in the observing telescope, obtained from the photometer lamp for each decimeter of the lamp-scale, was next constructed from data obtained by observation. The assumption which has been made in similar photometric measures that the intensity of the illumination is inversely proportional to the square of the distance of the lamp-flame from the slit, leads to results which may be considerably in error, particularly if some of the observations were made when this distance was small. The reasons for this are various. If, starting with the lamp at the end of its scale, we slide it gradually forward toward the slit, the intensity of the light in the observing telescope will increase gradually until the aperture of the collimator is filled, and then on closer approach the intensity no longer increases but remains constant, whereas by the law of inverse squares it should increase from a certain value up to infinity at the slit. In the apparatus used in these experiments this constancy of illumination began at about 2 or 3 decimeters, and measurements made with a smaller scale reading than 5 decimeters were avoided as much as possible. On account of the small proportion of blue and violet rays in the lamp-light, however, it was sometimes necessary to make the comparisons in the upper end of the spectrum with the lamp so near the slit that the value of its light intensity was subject to considerable uncertainty, and it is for this reason that the great difference in quality between the standard and the lights to be compared is so prejudicial to the accuracy of the observations.

It was preferred, in making the measurements, to diminish as much as possible the violet of the sunlight or moonlight by means of the wheel-photometer, thus enabling the comparison to be made with the lamp at a greater distance from the photometer slit.

The edges of the lamp-flame are considerably more brilliant than the central portions. When the lamp is near the extremity of its scale, its effective brilliancy is the average of that of all its parts, but when brought up close to the slit, the effective rays are those from the central portions only. From both this and the foregoing reason, the decrease in the brilliancy of the light in the observing telescope as the lamp is moved away from the slit is less rapid than that required by the law of inverse squares.

The law actually followed was determined empirically by means of the wheel-photometer. A second kerosene lamp with Argand burner, quite similar to the standard lamp, was placed directly in front of the slit, at such a distance that when matched by the standard lamp the scale reading of the latter was a little less than 5 decimeters. Having determined this reading by taking the mean of five settings, the wheel-photometer with its index set to 10 (or with its apertures open to their full width), was interposed between the auxiliary lamp and the slit, cutting down the brilliancy of the direct light to one-half; and the new position of the photometer lamp, when matched with the diminished auxiliary, was determined as before by five settings. The index of the wheel was then set to 9, reducing the intensity of the direct light to nine-twentieths, and so on until the reduction amounted to two-twentieths, when the limit of the lamp-scale was reached.

The following observations were made on November 5, 1884, each position of the photometer lamp being the mean of five independent settings, which sometimes, though very rarely, differed from each other by as much as 1 decimeter, the usual variation being from 1 to 5 centimeters. The comparisons were made in the yellow, experience having shown that equality was most accurately judged of in that color.

Setting of wheel-photometer.	Intensity.	Reading of lamp scale.
		Decimeters.
No wheel	1.00	4.94
Wheel index at 10	.50	7.60
Wheel index at 9	.45	8.06
Wheel index at 8	.40	8.34
Wheel index at 7	.35	8.90
Wheel index at 6	.30	9.74
Wheel index at 5	.25	10.84
Wheel index at 4	.20	12.34
Wheel index at 3	.15	14.76
Wheel index at 2	.10	18.34
No wheel	1.00	4.70

These observations, when plotted, give points which fall very nearly on a smooth curve. From this curve we may then take the intensity corresponding to even decimeters on the lamp scale, the unit of intensity being one-twentieth of the intensity of the auxiliary lamp. Finally, we may express the intensity in terms of another purely arbitrary unit; namely, that of the standard lamp at a distance of 5 decimeters from the slit. We thus obtain the following table. The last column is the adopted value of the lamp-intensity at each division of the scale, obtained by taking the mean of this and another similar set of observations.

Scale reading.	Intensity in twentieths.	Intensity.	Adopted intensity.	Scale reading.	Intensity in twentieths.	Intensity.	Adopted intensity.
Decimeters.				*Decimeters.*			
5	19.1	1.00	1.00	13	3.7	.19	.20
6	15.2	.80	.81	14	3.3	.17	.18
7	11.9	.62	.63	15	2.9	.15	.16
8	9.1	.48	.56	16	2.6	.14	.14
9	6.8	.36	.38	17	2.3	.12	.12
10	5.7	.30	.32	18	2.1	.11	.11
11	4.9	.26	.27	19	1.9	.10	.10
12	4.2	.22	.23	20	1.7	.09	.09

Plate 3 is a curve representing the intensity of the photometer lamp as a function of the scale reading, as determined by the experiments, and also the curve (dotted), on the assumption that the intensity varies inversely as the square of the distance from the slit. The less rapid decrease of intensity by the actual law is apparent. The unit of intensity in the last column, namely, that of the photometer lamp, at 5 decimeters from the slit, will be used throughout *for all colors*, no matter what their relative proportions in the lamp-light may be. The observations made on the moon on November 2, 1884, and those on the sun November 7, 1884, are given in full below. The observations of November 2, 1884, on moonlight, were made between the hours of 10 and 11 p. m.

Photometric observations on moonlight.

[Observer J. E. Keeler. All conditions favorable. The sky "at first slightly hazy, gradually becoming perfectly clear." At the time (10 h. 30 m.) the moon's hour angle was 1 h. 3 m. and her declination + 10°, corresponding to a zenith distance of 34° and air-mass M=1.21.]

Setting.	Color.	Reading of lamp-scale.					Mean.	Remarks.
° ′								
84 5	Deep red	12.2	13.0	12.1	10.9	11.9	12.0	Wheel before lamp, index at 3.
84 47	Bright red	11.4	10.4	10.7	10.1	9.9	10.5	Do.
85 40	Orange	8.4	7.6	7.9	7.7	8.0	7.9	Do.
85 52	Yellow	9.7	8.6	8.8	8.7	8.8	8.9	Wheel index at 5.
87 8	Green	6.2	5.9	6.3	5.6	5.6	5.9	Do.
87 8	do	11.6	12.4	12.1	11.7	12.2	12.0	No wheel.
87 42	Blue	8.9	8.5	8.8	8.2	8.4	8.6	Do.
88 0	Bright violet	6.7	6.6	6.9	6.4	6.1	6.5	Do.
89 0	Deep violet	3.4	3.0	2.4	2.3	2.9	2.8	No wheel; faint.

These observations are reduced with the aid of the table of lamp-light intensities on page 28. In the following table the last column contains the intensity of moonlight in different parts of the spectrum, as compared with the standard lamp:

Wave-length.	Mean lamp setting.	Tabular intensity.	How modified by wheel.	Actual intensity.
μ				
0.687	12.0	.230	Reduced to $\frac{3}{20}$	.035
0.649	10.5	.295	do	.044
0.599	7.9	.513	do	.077
0.586	8.9	.392	Reduced to $\frac{1}{4}$	.098
0.518	5.9	.829	do	.207
0.518	12.0	.230	Not modified	.230
0.486	8.6	.428	do	.428
0.470	6.5	.720	do	.720
0.415	2.8	....	do	....

In the observations of November 7 the sun was near the meridian (exact time not noted), and his declination being —16°, his zenith distance was about 56°, corresponding to an air-mass of M=1.79. The condition of the apparatus was the same as in the previous measures. The sky was a "fair hazy blue."

Photometric observations on sunlight.

Setting.	Color.	Reading of lamp-scale.					Mean.	Remarks.
° ′								
84 5	Deep red	12.2	13.1	12.0	11.2	11.2	11.7	Wheel before lamp, index at 5.
84 47	Bright red	9.0	8.7	8.6	8.0	8.8	8.6	Do.
85 40	Orange	15.4	15.7	16.2	14.8	15.0	15.4	No wheel.
85 52	Yellow	13.5	11.7	12.9	12.7	11.6	12.5	Do.
87 8	Green	6.0	6.2	6.2	6.2	6.5	6.2	Do.
87 42	Blue	8.9	9.5	9.6	9.7	9.6	9.5	Wheel before sun, index at 5.
89 0	Bright violet	8.2	8.0	8.6	8.2	8.4	8.3	Do.
89 0	Deep violet	6.0	5.1	5.5	6.0	5.6	5.6	Do.

These observations are reduced in the same way as those given in the first example. It is evident that introducing the wheel photometer in the path of the sunlight increases the value of the light intensity at a given division of the lamp-scale by the same factor that it is diminished when the wheel is interposed between the lamp and the slit.

Wave-length.	Mean lamp setting.	Tabular intensity.	How modified by wheel.	Actual intensity.
μ				
0.687	11.7	.242	Reduced to $\frac{1}{4}$	.061
0.649	8.6	.428	....do	.107
0.599	15.4	.152	Not modified	.152
0.586	12.5	.215	....do	.215
0.518	6.2	.774	....do	.774
0.486	9.5	.350	Increased to 4	1.400
0.470	8.3	.464	....do	1.856
0.415	5.6	.886	....do	3.544

Two more such complete sets of observations were made under favorable circumstances, one on the sun on November 1, and one on the moon on October 31. Other observations made under disadvantageous circumstances, such as a hazy or smoky sky, were rejected.

The differences, which are sometimes considerable, between those results of these observations which should be identical, are due to errors of observation, as well as to different conditions of the atmosphere at the times of observations, differences in altitude of the heavenly bodies observed, and variations in the intensity and quality of the light from the photometer lamp. Since the effects of these sources of error, with, perhaps, the exception of that due to difference in altitude, do not allow of computation, the best we can do is to regard them as made under perfectly similar circumstances and combine them accordingly. We shall then at least know, from a consideration of the general effect of the actual differences in circumstances, in which direction the error of the combination lies.

The following table exhibits the mean values resulting from such a combination. In the last three columns are given the intensities of the three kinds of light in terms of lamp-light, *all being supposed equal in the yellow.* The fifth and sixth columns are obtained by multiplying the second and third columns throughout by a proper factor:

Wave-length.	Moonlight.	Sunlight.	Lamp-light.	Moonlight.	Sunlight.
μ					
0.687	.032	.055	1.00	.41	.26
0.649	.041	.096	1.00	.52	.46
0.599	.066	.165	1.00	.84	.79
0.586	.079	.209	1.00	1.00	1.00
0.518	.190	.696	1.00	2.41	3.33
0.486	.370	1.092	1.00	4.69	5.22
0.470	.592	1.890	1.00	7.50	9.03
0.415	1.050	3.540	1.00	13.29	16.92

Plate 4 is a graphical reprsentation of this table. The intensity of lamp-light is represented by a straight line everywhere at the distance 1 from the axis of λ. The sunlight and moonlight curves intersect this line at the point $\lambda=0^{\mu}.586$. They rise rapidly towards the violet end, but the sunlight ordinates increase faster than the moonlight ones. These curves show that the proportion of violet in sunlight is much greater than in moonlight, although as a quantitative determination the observations are not entirely satisfactory. The principal cause of error is, as already mentioned, the deficiency of violet rays in the light from the comparison lamp. The errors of observation become more apparent on eliminating this intermediate term, and comparing directly the light from the sun with that from the moon. From the curve in the figure we obtain the first part of the following table, and by a graphical construction of this part we get the last two columns from a smooth curve. This curve, as given by the table, is concave towards the axis of λ. It is quite certain, however, that if the observations had been perfect and made under similar circumstances it would be convex. It is evident that the sunlight in these measures is at a great disadvantage in respect to the moonlight, especially in the upper regions of the spectrum, since the violet light from the sun, which was observed at a much lower altitude, had been more powerfully absorbed by the atmosphere. This absorption was even greater than could be expected from the mere difference in altitude, for the sky at night was almost invariably better than in the daytime, and, moreover, the cloud of smoke, which always hangs over the city of Pittsburgh towards the south, gives an absorption for large zenith distances much greater than the mass of air traversed would produce alone.

If the observations had been made under precisely similar circumstances, the preponderance of violet in the solar spectrum would be far more pronounced.

Wave-length.	$\frac{\text{Sunlight.}}{\text{Moonlight.}}$	Adopted $\frac{\text{Sunlight.}}{\text{Moonlight.}}$	Adopted $\frac{\text{Moonlight.}}{\text{Sunlight.}}$
μ			
0.687	.65	.68	1.47
0.649	.88	.81	1.23
0.599	.95	.96	1.04
0.586	1.00	1.00	1.00
0.518	1.23	1.18	.85
0.486	1.35	1.26	.79
0.470	1.35	1.31	.76
0.415	1.28	1.43	.70

In addition to this table we give another, containing the results obtained by different observers, reduced by intepolation from smooth curves to the same points measured on in 1884. As some of these measurements were made under circumstances exactly opposite, as regards the relative heights of the sun and moon, to those we have described, we may expect from a combination of them all to obtain a result more nearly free from the effects of unequal absorption of the light from the two bodies by the atmosphere.

Relative intensities of sunlight and moonlight.

Wave-length.	Pickering.	Preliminary observations of 1883.	Observations of 1884.	Vogel.	Mean* by weights.	$\frac{\text{Moonlight.}}{\text{Sunlight.}}$
μ						
0.687	.48	....	.68	.90	.70	1.43
0.649	.64	....	.81	.92	.77	1.30
0.599	.89	.7	.96	.98	.92	1.08
0.586	1.0	1.0	1.00	1.00	1.00	1.00
0.518	2.2	6.5	1.18	1.26	1.68	.60
0.486	4.6	9.5	1.26	1.40	2.37	.42
0.470	6.3	13.	1.31	1.54	2.72	.37
0.415	13.	20.	1.43	2.10	4.22	.24

* The values in the sixth column have been used as ordinates for the curve. (Plate 5.)

In obtaining the column headed "Mean" the weight 5 has been given to each of the two preceding columns and the weight 1 to each of the others. The observations of Mr. PICKERING on the moon were made under unfavorable circumstances, and the light ratio in the violet depends upon a single series of three readings. Those made at this observatory in 1883 were for the purpose of experimenting on the best arrangement of apparatus, and not made with a view to obtain the best quantitative results, while the values given by Dr. Vogel and by the Allegheny observations of 1884 are the results of many and careful observations throughout the entire range of the spectrum. The weight which we have assigned to them would not therefore appear to be too great.

With the aid of this table we may make an effort to draw the lunar energy curve. Within the limits of our observations an increase in energy in a definite part of the spectrum is followed by a proportional increase in brilliancy,* so that the figures in the last column, which represent

* With intenser lights than we employ certain physiological phenomena affect this proportionality, which is here, however, sensibly exact.

the light intensity ratio of moonlight and sunlight, may also be taken as the ratio of the ordinates of the lunar and solar energy curves.

For the ordinates of the normal solar energy curve we may take the values given by the mean of all noon observations made with the spectro-bolometer at Allegheny during the spring of 1881. Then, multiplying each ordinate by the corresponding factor given in the last column of the preceding table, we obtain the ordinates of the lunar energy curve. The results are exhibited below in tabular form:

Wave-length.	Solar ordinates.	Lunar energy. / Solar energy.	Lunar ordinates.
μ			
0.687	575	1.43	822
0.649	604	1.30	785
0.599	624	1.08	674
0.586	622	1.00	622
0.518	590	.60	354
0.486	535	.42	225
0.470	490	.37	181
0.415	300	.24	72

If we wish the lunar energy curve to represent the distribution of the same amount of energy as the solar within the limits of the visible spectrum (say between $0^\mu.4$ and $0^\mu.7$), we must multiply each of the lunar ordinates by the fraction $\frac{329}{284}$, which is determined by plotting the curves and measuring the areas within the required limits. We obtain by this operation the following table, which is also graphically represented by the curves in Plate 6.

Wave-length.	Solar ordinates.	Lunar ordinates.
μ		
0.687	575	952
0.649	604	909
0.599	624	780
0.586	622	721
0.518	590	410
0.486	535	261
0.470	490	209
0.415	300	83

An inspection of these curves shows at once the effects of the selective absorption undergone by the solar rays at the moon's surface. The maximum ordinate of the lunar curve falls much lower down in the spectrum, and there is a corresponding reduction in the height of the curve over the violet end. The visible part of the normal spectrum forms, however, so small a portion of its entire length, that it would be unsafe to judge from the nature of the lunar curve obtained by optical means, as to its probable course at points very far below the limit of the visible red. Nevertheless, the evidence of these photometric measurements as to the selective reflection exercised by the moon's surface is, as far as it goes, decisive, and it is shown to be in such a direction as to cause a preponderance in the lunar spectrum of the rays of long wave-length, and hence to tend to cause a smaller percentage of lunar rays to be transmitted by glass than of solar, and this independently of any effect from heat reradiated by the lunar soil. There is, then, no doubt that the observed phenomenon of glass absorption already described is due in part to this cause, though in how large part we do not now determine.

ADOPTED HEAT-MEASURES WITH BOLOMETER AND GALVANOMETER.

The galvanometer is so important an accessory of the bolometer, that we will describe the arrangement we have used to make our own most effective.

The galvanometer employed is a Thomson differential astatic galvanometer, having a resistance of 20.35 ohms, and originally made by Elliott Brothers, with a short suspending fiber, a damping magnet sliding on a brass rod, and a system of five upper and five lower magnets connected by an aluminum rod with an aluminum vane, the time of a single vibration without damping magnet being 6.58 seconds.

In preparation for the extremely delicate final work on the moon, the following changes were made: (I have to express my great obligations to the kindness of Prof. Sir William Thomson and of Professor Rowland for valuable suggestions.) The most important of these improvements has been the replacing of the short fiber by one 33 centimeters in length (for the brass rod being substituted a hollow glass one, in the center of which is the fiber); and, second, the reconstruction of the needle.

In the new astatic system constructed at this observatory in November, 1884, the aluminum rod carrying the magnets was replaced by a hollow glass fiber. The aluminum vane, it occurred to me to replace by an insect's wing, and one was most advantageously made of dragon-fly's wings, (in which nature has supplied an admirably rigid and light construction). A minute platinum paddle at the bottom of the glass fiber, touching the surface of oil in an oil-cup, was supplied, and a new system of magnets. These are made by rolling soft sheet-steel, 0.076 millimeters thick and 5 millimeters wide and from 7 to 9.5 millimeters long, around a short straight piece of wire into minute cylinders, carbonizing them in fused ferrocyanide of potassium, and tempering them in mercury. The strength of one of the little magnets was found to be 874 Gaussian units, and of these there are in all twelve, six on each system.*

In forming the connections, it will be found advantageous to employ a battery of a considerable number of cells (*e. g.* twelve, of a gravity battery), and to reduce the current by interposing resistance. Under these circumstances, it might appear that there was no advantage in using the current from twelve cells over that of one, if the current were as strong in either case. Such, however, is not the fact; for the accidental fluctuations due to the minute casual changes which take place in the most constant cell are obviously equalized by the use of a current which is the mean of that from a considerable number of cells.* Pains are taken to wrap every connection and binding post in cotton, and a great number of minute precautions, which are not here detailed, have been observed.

The damping magnet is arranged so as to take any position between the bottom of the glass rod and a point 1.46 meters above it, a graduated vertical scale being provided above the galvanometer rod. The mirror of the instrument is a minute silver-on-glass concave reflector, of 1-meter radius of curvature. The transparent scale, which is on the west, at 1 meter distance, is a portion of a cylinder of 1-meter radius, and is graduated in millimeters from 0 to 500. Accordingly, when the needle points north and south, and the optical axis of the mirror east and west, the image is at 250, at the middle of the scale. This image is a circle of light about 3 centimeters in diameter, with a central vertical line (the shadow of a wire).† With these values, to carry the image wholly off the scale demands a rotation of the needle through only about 7 degrees. As a rule, this small maximum deviation, with the employment of a curved scale, renders reduction for arc unnecessary in such observations as these. The needle, when rendered as astatic as possible, performs a single vibration in about a minute; but in this condition the directive force is apt to vary from one day to another, and the time of vibration, as a rule, to grow more rapid until a shorter period is reached, at which it becomes relatively constant. For the purpose of forming an approximate estimate of the sensitiveness of the instrument, it may be stated that when making a single vibration in 10 seconds a deflection of one millimeter division on the scale is given by a current approximately equal to 0.0000000013 ampère.

East of the galvanometer, and nearly in the prolongation of the optical axis of the upper mirror, are two small bar magnets, on an independent stand, a minute movement of either of which serves to bring the image on to any point of the scale when necessary without altering the resistance in the resistance box.

* The device of the hollow magnets is due to Mr. F. W. Very, of this Observatory, at whose suggestion also the number of battery cells was increased with great advantage. The actual construction and astaticising of the needles also has been chiefly due to Mr. Very's patience and skill.

† The employment of a telescope and a flat mirror, reflecting the inverted scale, is in some respects preferable to this arrangement, which is continued in use, however, at present from its greater facility of adjustment.

The adjustments are commonly made so that heat falling upon the bolometer causes a deflection of the image to the south, thus increasing the reading on the scale, whose zero is at the northern end. It may be added, in further indication of the sensitiveness of the instrument, that on bolometer No. 1 (whose resistance is 80.5 ohms) by Matthiessen's table, the change of temperature, corresponding to a change of resistance of 0.0001 ohm, is 0°.00032 C. Accordingly, when the needle is in such a condition of sensitiveness that it executes a single vibration in 10 seconds, and if we employ a current of 0.1 ampère, a change of one division on the scale corresponds to a change of temperature in the bolometer strips of 0°.000016 C. This result is to be understood as merely approximate, and as indicating nearly the limit of sensitiveness attained in actual work at present. It need hardly be added that greater nominal sensitiveness can be obtained to almost any extent by increasing the time of swing; but the gain is apt to be only nominal, for we are to consider that, other things being equal, the efficiency of the instrument increases as the probable error diminishes, where this probable error is expressed as a fraction of the deviation in question. In fact, as the concentrated moonbeam drives the image off the scale altogether in the above condition of sensitiveness, it is necessary to employ the damping magnet, not to increase, but to diminish, the time of vibration, so that the image may remain on the scale. Under these latter conditions the probable error of a single observation is very small, probably not exceeding 2 per cent.*

DESCRIPTION OF BOLOMETERS EMPLOYED.

Bolometer No. 1, which has been chiefly used in measurements of total lunar radiation when concentrated by the concave mirror, and for comparative observations with the Leslie cube, has a square central aperture, 8.3 millimeters on a side, through which the blackened strips of the central or exposed arm may be seen, presenting to the incoming rays an area of 49 square millimeters, and composed of 23 thin strips of blackened platinum, each about 0.001 millimeter thick, in two tiers, the rear ones covering the apertures left between the front ones. The other, or protected arm, is made up of 24 strips, an extra protected strip being introduced in the circuit of the exposed arm, to balance the resistance, which is 80.5 ohms for either arm. The case is a cylinder of ebonite, projecting so far beyond the strips as to cut them off from all radiations, except those from the subject of experiment.

Bolometer No. 13 is composed of 8 side strips and 9 central ones, each 0.25 millimeter wide, the latter forming a band 2.3 millimeters wide and 10.3 millimeters high, with which measures in the lunar spectrum have chiefly been made. Each arm resists 38.4 ohms.

Our direct observations on the lunar heat may be grouped under six divisions. (1) Quantitative measurements of lunar heat as compared with solar; (2) comparisons of the moon's heat with that from a terrestrial source; (3) the comparative transmissibility of our atmosphere for lunar and solar heat; (4) comparative transmissibility of glass for lunar and solar heat; (5) heat observations during a lunar eclipse; (6) the formation of a lunar-heat spectrum.

CLASS 1.—QUANTITATIVE MEASUREMENTS OF LUNAR HEAT AS COMPARED WITH SOLAR.

Let us expose the bolometer to the lunar radiation, either direct or concentrated, and note the resultant galvanometer deflection, and repeat the experiment the next day with the solar radiation, diminished in a known ratio. If the moon be full and at an equal altitude with the sun at the time of observation, we have the direct ratio of heat received from each at the earth's surface but it is to be remarked that we cannot confine these observations to the single night of full moon without giving inordinate time to the research (since they should be often repeated); while, if we take them at times much before or after the full, considerable errors may be introduced by our ignorance of the true law of the variation of the moon's heat with the phase. Where we have been obliged to use the latter class of observations, we have reduced them by Zöllner's law. It is to be observed, also, that it is not only more than doubtful whether the transmissibility of the atmosphere

* It appears in Lord Rosse's observations that the mean of a series of 10 gave a probable error of 19 per cent. with the thermopile and galvanometer then employed. Accordingly, if we do not consider constant errors, but only accidental ones, we find that, owing to the increased sensitiveness and steadiness of our apparatus, a single measurement with the present train is equivalent to several hundred of that employed by Lord Rosse.

is the same by night as by day, but that other circumstances add to the difficulty in forming exact conclusions.

The apparatus employed in the following observations for lunar rays consisted of the concave mirror and bolometer shown in Plate 1. These were used at night, while in the day the sun's rays passed through a narrow aperture and fell on the bolometer placed at a considerable distance in the divergent beam.

The following are the principal constants of the apparatus:

Let S=lunar apparent semi-diameter at the time of observation, obtained from the geocentric semi-diameter corrected for augmentation.

f=the focal length of concave mirror=73.4 centimeters.

A=the radius of lunar beam falling upon the concave mirror=10.2 centimeters. This radius is limited by that of the hole in the north wall by which the beam from the siderostat enters.

Let s be the semi-diameter of the lunar image in the focus of the concave mirror. Then

$$f \sin S = s, \text{ and } \frac{A^2}{s^2} = \text{concentration of lunar beam.}$$

The absorption by the silver of the mirror and the loss by non-perpendicular incidence of the rays on the bolometer strips are here neglected.

Again, let a be the semi-diameter of the aperture used for transmitting the solar rays, S′ the solar semi-diameter, and d the distance of the bolometer strips from the diaphragm. Then $\pi (d \sin S' + a)\,{}^2\pi l^2$, where l is the radius of the circle formed by the divergent solar beam at the distance of the bolometer strips, and, neglecting the effect of diffraction at the diaphragm, the diminution of the solar beam $= \frac{a^2}{l^2}$. The ratio $\frac{A^2}{s^2}$ to $\frac{a^2}{l^2}$ is that of sun heat to moon heat, or rather that from an element of the center of the sun's surface to the mean value of the heat from the full moon. Observations of this kind were made on the following evenings, November 29, December 2, and December 3, 1884.

As an example we give that of December 2, 1884:

Time.	Zenith distance.	Deflection.	Time.	Zenith distance.	Deflection.
h. m.	° ′		h. m.	° ′	
6 24	72 24	174	9 39	38 24	303
6 36		188	9 41		312
7 59	54 22	273	9 45		311
8 15		285	9 47		307
8 21	52 05	295	9 51		305
8 24		301	9 55		317
8 27		311	11 58	22 22	326
8 30		298	12 02		332
8 32		297	12 04		315
8 35		309	12 07		331
			12 09		313
			12 13		318

The sky during these observations was quite good and cloudless. A light haze having gathered shortly after midnight, observations were discontinued before the moon had quite reached the meridian.

On the following day (December 3) observations were made on the sun at noon. The state of the sky appeared to be about the same as during the preceding lunar observations, and the battery current employed was adjusted to the same strength.

The following observations were obtained:

Sun's hour angle.	Deflection.
min.	
−10	163
− 7	181
+ 7	170
+10	196
+15	174
	Mean..176

One hundred and seventy-six divisions was therefore taken as the deflection produced by the sun at apparent noon, when his zenith distance was 62° 43′.

On drawing a smooth curve through points given by the lunar observations in the first table, we find that the deflection produced by the moon at the same zenith-distance was 245 divisions. On the evening of December 2 the moon's geocentric semi-diameter was 16′ 47″, and the semi-diameter at the time and place of observation was 16′ 55″. The focal length of the concave mirror being 73.4 centimeters and the diameter of the moonbeam 20.3 centimeters, the concentration of the moonlight was

$$\frac{(20.3)^2}{(73.4 + \sin 16' 55'')^2} = \frac{(20.3)^2}{(.722)^2} = 790.5$$

. The aperture through which the sunlight was admitted was 0.486 centimeter diameter, and the bolometer strips, when exposed, were distant from it 653.5 centimeters. The sun's semi-diameter at noon being 16′ 16″.5, the approximate diminution of solar light and heat was

$$\frac{(0.486)^2}{(653.5 \times \sin 16' 16''.5)^2} = \frac{(0.486)^2}{(6.674)^2} \quad .00530$$

The moon was not quite full at the time of observation. We find by Zöllner's formula that if it had been, the deflection produced would have been $\frac{245}{.90}=272$ div. We have, then, for the observed ratio of radiation from the full moon to that of the sun, both bodies being at a zenith distance of 62° 43′,

$$\frac{272}{176} \times \frac{1}{790.5} \times \frac{530}{100{,}000} = \frac{1}{96509}$$

values which the reader is again reminded are presumably subject to large constant errors. The maximum total heat which we can by possibility receive from the moon, even in the absence of an absorbing atmosphere, as is shown elsewhere, is about 1-97000 of the solar heat. It is improbable that such a coincidence as that presented with the observed value just given is other than largely the result of chance, or rather of such constant errors tending in an unknown degree to increase the observed values.

CLASS 2.—COMPARISON OF THE HEAT FROM THE MOON WITH THAT FROM A LESLIE CUBE.

On December 3, 1884, the temperature of the room being 0° C., the bolometer was exposed to the radiation from a Leslie cube filled with boiling water, which was observed through the circular aperture of a screen subtending the same angle as the cone of rays from the concave mirror used in lunar measurements. The following deflections were observed, the sensitiveness of the measuring apparatus being the same as during the lunar observations of the previous evening:

Temperature of Leslie cube.	Galvanometer deflection.
° C.	Div.
95	408
92	400
89	384
86	370
83	369
81	353
73	297

From a smooth curve we adopt 435 as the presumable deflection, which would have been observed under these conditions at 100° C. The screen itself acquired a minute amount of heat during the experiment, but the correction for this is negligible.

The bolometer strips attain thermal equlibrium under ordinary circumstances within a fraction of a second, while on account of the slowness of change of the temperature of the case, we can assume its radiation (C) to be constant during the experiment. The temperature of the bolometer strips may always be taken to be proportional to the angular area of the part of the surface radiating to them, to its temperature, and to its emissive quality.

Thus the aperture of the moon bolometer occupies 0.00565 of the sphere. The temperature of the room December 3, 1884, was 0°.0 C. If the aperture had pointed to a surface at the absolute zero, having the same emissive power as its case, a fall of temperature of 0.00565 multiplied by $273^\circ = 1^\circ.542$ would have been experienced. We assume that, within the limits of this experiment, the Newtonian law of radiation holds. If the pointing had been to a surface at 100° C. of the same emissive power, the temperature of the bolometer would have risen 0°.565. Now, we have seen that a Leslie cube at 100° C. would have produced a deflection of 435 divisions on the galvanometer. One division, therefore, indicated $\frac{0^\circ.565}{435} = 0^\circ.0013$ change of temperature of the bolometer strips (the full sensitiveness of the galvanometer not being used). The deflection produced by the full moon on the previous evening, if reduced to zenith, would have been over 350 divisions, and the temperature of surrounding objects being 0° C. the effective radiation of the moon, if we suppose its emissive power the same as that of the case, was such as would correspond to a temperature of $\frac{350}{435} \times 100^\circ = +80^\circ.5$ C., or 80°.5 C. above the temperature of surrounding terrestrial objects, which happened to be zero Centigrade. This on the absolute scale gives $80^\circ.5 + 273^\circ = 353^\circ.5$; and if one-fourth of the lunar radiation is reflected sun heat the true average temperature of the moon at the full is $80^\circ.5 - \frac{1}{4} \times 353^\circ.5 = -7^\circ.9$ C. If we assume that one-half only is reflected sun heat, we have $-99^\circ.3$ C., if that one-sixth is reflected, $+ 21^\circ.6$ C.

A correction for atmospheric absorption which we have not applied would somewhat increase these values, but it is evident that not only the experimental conditions here do not favor accuracy but that the results, such as they are, are subject to a wide latitude of interpretation.

CLASS 3.—TRANSMISSION OF LUNAR HEAT BY THE EARTH'S ATMOSPHERE.

The remarks already made as to the difficulty of comparing observations at different altitudes but at different phases, when the law of change of heat with the phase is so imperfectly known, apply with peculiar force to this class of observations. Only a series of observations made exclusively on the full moon in favorable circumstances (and therefore occupying many years) could bring anything like satisfactory evidence. The reductions which we now give of the few values we possess lead to conclusions to which we cannot attach great weight.

The observations of December 2, 1884, are given below in tabular form, with the computations necessary for obtaining the coefficient of transmission by the atmosphere. This coefficient, as has been explained elsewhere*, is found by means of the formula

$$\log t = \frac{\log d_2 - \log d_1}{M_2\,\beta_2 - M_1\beta_1}$$

and from this may be found the original energy of the observed radiation before it entered the atmosphere,

$$\log E = \log d_1 - M_1\,\beta_1 \log t$$

although these formulæ are strictly applicable only to homogeneous rays, and hence give only approximate results. Each "deflection" is the mean of a number of observations made nearly at the same time.

Lunar heat observations of December 2, 1884.

[Height of barometer β=7.34 decimeters.]

Observation.	Time.	Hour angle.	Zenith distance.	M.	$M\beta$.	Deflection.	Deflection corrected for change of phase.
	h. m.	*h. m.*	° ′				
g	6 24	5 39	74 28	3.68	27.01	174	183
f	6 36	5 28	72 24	3.28	24.08	188	197
e	7 59	4 09	57 25	1.865	13.69	273	284
d	8 15	3 53	54 22	1.718	12.60	285	294
c	8 28	3 41	52 05	1.627	11.94	302	311
b	9 46	2 26	38 24	1.277	9.37	309	315
a	12 05	0 13	22 29	1.080	8.00	323	323

The comparison of observations made at great and small zenith distances is also exhibited in the form of a table.

Observations compared.	d_1	d_2	Log d_1	Log d_2	Log $\frac{d_2}{d_1}$	$-\text{Log}\left(\frac{\text{Log } d_2}{\text{Log } d_1}\right)$	Log $(M_2\beta_2 - M_1\beta_1)$	Log (Log t)	Log t	t	$-M_1\beta_1$ Log t.	Log E.	E.
a and *g*	323	183	2.5092	2.2625	−.2467	9.3921	1.2790	8.1131	−.0130	.971	.1040	2.6132	410
a and *f*	323	197	2.5092	2.2945	−.2147	9.3318	1.2062	8.1256	−.0134	.970	.1072	2.6164	413
a and *e*	323	284	2.5092	2.4533	−.0559	8.7474	0.7551	7.9923	−.0098	.978	.0784	2.5876	387
b and *g*	315	183	2.4983	2.2625	−.2358	9.3726	1.2465	8.1261	−.0134	.970	.1256	2.6239	421

The average value of t is 0.972, which is the fraction of the lunar radiation transmitted by a column of air capable of supporting 1 decimeter of mercury. The fraction of a vertical beam transmitted by the entire depth of the atmosphere would be $t^{7.6}$=.806.

The correction due to the change of the phase of the moon during the course of the night's observations is taken from a curve based on the formula of Zöllner.

CLASS 4.—COMPARATIVE TRANSMISSION OF GLASS FOR LUNAR AND SOLAR HEAT.

The pieces of glass used were the same as those employed in the preliminary experiments. They were A, B, and the "large window pane." A series of observations made by moving the siderostat mirror so as to expose alternately to the adjacent sky and to the moon gave, as has already been said, systematically different results from those obtained by the interposition of a screen and other modes of observation. For reasons already given, the values found by alternate exposure to the moon and sky are preferred. We give as an example the observations of December 3, 1884, on the sun, and of November 26, 1884, on the moon, the general disposition of the apparatus em ployed being that indicated in plate 1, and the glass, thoroughly dried and cleaned, being in ev-

*American Journal of Science, Vol. 125, page 176.

ery case allowed to communicate its own temperature to the bolometer before being exposed to the lunar or solar rays.

Transmission of solar radiation by glass.

[December 3, 1884. Sun's zenith distance at apparent noon 62° 43′. Sunbeam diminished in intensity to about 0.00530. The transmission is obtained by dividing the deflection, when the rays pass through glass, by the mean of the adjacent deflections in the direct solar beam.]

Mean time.	Deflection in direct solar beam.	Deflection with glass interposed.	Transmission of solar radiation by glass
h. m.			
11 40	163		
11 42		125	0.727
11 43	181		
11 57	170		
11 59		133	0.727
12 00	196		
12 05	174		
12 07		130	0.791
12 08	155		
12 10		123	0.794
12 12	155		
12 13		112	0.725
12 15	154		
12 16		119	0.761
12 17	159		
12 19		118	0.735
12 20	162		
		Mean.	0.751

Transmission of lunar radiation by glass.

[November 26, 1884. A good sky. External temperature—5°. 0 C. Barometer, $\beta = 7.26$ decimeters.]

Mean time.	Moon's distance from meridian.	Moon's zenith distance.	Air mass. $M\beta$.	Deflection in direct lunar beam.	Deflection through glass.		Transmission.
					Glass outside.	Glass inside.	
h. m.	*h. m.*	°					
5 56	0 48			240			
6 00	0 44	$44\frac{1}{2}$	10.2		22		.090
6 07	0 38					35	.139
6 11	0 34			257			
6 19	0 26				29		.114
6 24	0 21					30	.119
6 31	0 14			251			
6 53	0 06	$43\frac{1}{2}$	10.0	260			
7 00	0 13				26		.102
7 04	0 17				(2 layers=18)		
7 07	0 20					28	.112
7 14	0 28	44	10.1	247			
7 19	0 33				34		.140
7 25	0 39					30	.127
7 31	0 42	$44\frac{1}{2}$	10.2	231			
9 12	2 20	53	12.1	227			
9 17	2 25	54	12.3		29		.135
9 21	2 29	$54\frac{1}{2}$	12.5			26	.126
9 26	2 33	55	12.6	196			
9 33	2 40	56	12.9		26		.134
9 36	2 43	$56\frac{1}{2}$	13.1			25	.130
9 40	2 47	57	13.4	192			

Lunar radiation: Mean transmission (glass outside) .... .119
Mean transmission (glass inside) .... .126

Mean of all observations .... .122

There is very little difference between the results for transmission with glass inside and outside the building, and this little may be entirely accounted for by the diffusion of the rays by the

irregular surfaces of the glass pane, which produces larger loss when the latter is outside at a distance from the instrument.

The solar energy which falls on the moon may be divided into two portions: a, that which is reflected or diffused; b, that which is absorbed by the lunar soil and reradiated. We may form some rude *à priori* notion of the relative value of these from the following considerations: Were the full moon a perfectly diffusive body and reflecting according to the law established by Zöllner's experiments, it should behave nearly as a flat disc would do, and return to us such a portion of the sun's energy as the angular area of its disc bears to that of the hemisphere $\frac{1}{97300}$, $(\frac{1}{n})$. Hence we may take this fraction to express the ratio of total lunar radiation—*i.e.*, $(a+b)$—in terms of solar radiation. The ratio of lunar to solar luminous radiation is here taken to be (roughly) $\frac{1}{400,000}$—but the ratio of lunar nonluminous to solar nonluminous radiation, owing to the selective absorption of the lunar surface, is probably indefinitely greater. This latter ratio is unknown, but the larger it is the smaller is the portion which we must assign to radiated heat. If, for instance, the perfectly diffusive moon sends us $\frac{1}{n}$ of the total solar radiation, and the ratio of lunar to solar radiation within the limits of the solar spectrum be $\frac{1}{m}$, $\frac{n}{m}$ is the proportion which is diffused or reflected to us (a), and $1-\frac{n}{m}$ is that which is absorbed and radiated (b). Now, n is a little less than 100,000, and m varies with the degree of selective reflection in the lunar surface. If m be 600,000, one-sixth of the lunar radiation is reflected or diffused solar energy, and five-sixths absorbed and radiated from the soil. If m be 300,000, one-third of the energy is reflected, &c., and somewhere between these two values it seems probable that the ratio sought will lie. The heat sent earthward by the radiation from the lunar soil is almost certainly greater than that reflected or diffused; but our atmosphere is, according to what we have been hitherto accustomed to think, comparatively opaque to the first class of heat (that radiated from the soil) and comparatively translucent or diathermanous to the second, so that there seems an *a priori* probability that the true ratio between a and b, as it would present itself to an observer outside our atmosphere, will be altered by its absorption, and that actually observed at sea level be something different. It seems certain, at any rate, that the radiation of the lunar soil must be of a quality to which glass is nearly opaque, since the glass which we have employed in our own experiments is nearly opaque to the radiation from a source at 100° C.

CLASS 5.*—HEAT OBSERVATIONS DURING A LUNAR ECLIPSE.

The only lunar eclipse observed at Allegheny was that of October 4, 1884. The eclipsed moon rose behind clouds, and the first observation, obtained when the penumbra was already passing off, was made while the moon was still partly obscured by haze. Under these circumstances little interest attaches to the observation, which need not be cited in detail. The inference from it, so far as any could be drawn, was that about the same amount of heat was received as was to have been expected had there been no previous eclipse.

REVIEW.

Let us now review our sources of information and weigh the imperfect and sometimes contradictory results each has brought us.

(1) *Direct measurement of lunar heat as compared with solar.*—Our direct comparison indicates that we receive nearly the whole proportion of solar energy from the full moon that we should expect to get from a diffusive disk of the same angular aperture. This heat must in reality be partly diffused and partly radiated, and we do not know (from the present observations) in what proportions these two kinds enter. So far as the observation itself is reliable, we may, however, infer that our atmosphere is permeable to most of the lunar heat of either kind, but the method is unfortunately subject to such large sources of constant error, that we cannot derive great confidence from the apparent agreement of different observations or even of different observers. It may be said, however, to create a certain presumption that the earth's atmosphere is diathermanous to heat of lower wave-length than has been heretofore supposed, *and of lower wave-length than appears to reach us from the sun.*

* Class 6, see *infra*.

(2) *Comparison of moon's heat with that of Leslie cube.*—If we may draw any inference from this class of observations it is that the sun-lit surface of the moon is not far from the freezing temperature, but not so far below as we might expect to find that of an absolutely airless planet.

(3) *Transmission of lunar heat by the earth's atmosphere.*—Our observations indicate a not materially greater coefficient of transmission for lunar heat than for solar; and though their limited number and the uncertainty of the correction for change of heat with phase render more certainty as to the fact desirable, we may (accepting them as probable) reason thus.

Previous observations both at Allegheny and Mount Whitney have shown that the solar rays are transmitted with greater and greater facility (except for cold bands) as the wave-length increases up to the point (near $\lambda = 3^{\mu}$) where they suddenly disappear altogether. This shows either that (1) the solar heat, which according to the customary assumption exists to an unlimited wave-length before absorption, has here been cut off by a suddenly absorbent action, like that of a cold band extending indefinitely below 3^{μ}, or (2) that, either through a precedent absorption of such rays in the sun's own atmosphere or their non-existence, no solar rays below 3^{μ} present themselves to our atmosphere for admission.

The first view is that which I have treated as most in accordance with received opinion. It is not, however, the only one, since the second is not to be absolutely rejected, considering our experimental ignorance of the laws of radiation from gaseous bodies for great wave-lengths. Of these two hypotheses we see that, according to the first, our atmosphere is quite opaque to all heat below 3^{μ}, and the writer's (unpublished) experiments show that heat above this point must come almost wholly from a source much above 100° C. In this view, then (unless we agree that the radiations from the lunar soil correspond to a source much above 100° C.), we conclude that sensibly none of them pass our atmosphere, but that what we receive is diffused and reflected heat coming within the range of the known solar energy spectrum, and transmitted with nearly the same facility as solar heat, or if with a little greater, because lowered in wave-length by selective reflection at the lunar surface, not by absorption and reradiation from the lunar soil.

In the second view, for anything we have absolutely known to the contrary, our atmosphere may be permeable to radiations of any wave-length below 3^{μ}, and we could draw no certain inference, even if the lunar radiation were more distinctly different in transmissibility than it is.

As a matter of fact, with the actually limited difference in the character of its transmissibility, a difference which, as so far determined, is of the same order as that of the error of observation, we have no ground then from this present class of observation (*i. e.*, class 3) for any absolute conclusion one way or the other. But we repeat it seems to be a probable inference from our whole work that the earth's atmosphere is more diathermanous to heat of extremely low refrangibility than has heretofore been supposed.

(4) *Comparative transmission of glass for lunar and solar heat.*—The evidence here, which at first seems to so directly support the view of a sensible radiation from the surface of the moon, proves on examination to be subject to other interpretation, for the observed effect is almost certainly due in part to a degradation of wave-length by selective reflection from the lunar soil.

We can draw no absolute conclusion, then, from this evidence at first in appearance so promising, though we may say that it certainly indicates an increased probability for the view that radiations from the lunar soil may be transmissible by our atmosphere.

(5) *Observations during a lunar eclipse.*—If our own observations in this respect are imperfect, those of Lord Rosse before cited are on the other hand clear. They appear to bear but one interpretation, that all heat from the moon disappears immediately that it passes into the earth's shadow, and there is no evidence of any being retained, for any sensible time, more than if it were reflected.

It is so difficult to conceive that while the moon has been storing heat during many days of sunshine, it can part with it instantly, so that the temperature of the whole earthward surface of the planet disappears in an inappreciable interval, that most will see in this observation an argument against the existence of any such heat sensible to us at any time whatever.

(6) *Formation of a lunar heat spectrum.*—The observations made here with the lunar heat spectrum are as yet incomplete. With improving experience and apparatus, we hope to make others which shall give information of a character no other means can furnish. (See note, *infra.*)

CONCLUSION.

While we have found abundant evidence of heat from the moon, every method we have tried, or that has been tried by others for determining the character of this heat appears to us inconclusive; and, without questioning that the moon radiates heat earthward from its soil, we have not yet found any experimental means of discriminating with such certainty between this and reflected heat that it is not open to misinterpretation. Whether we do so or not in the future will probably depend on our ability to measure by some process which will inform us directly of the wave-lengths of the heat observed.

Note added February, 1885.—Since the above paragraph was written, we have succeeded in obtaining measures with rock-salt prisms and lenses in a lunar *heat spectrum*. These difficult measures must be repeated at many lunations before complete results can be obtained; but, considering their importance to the present subject, we think it best to state now in general terms, and with the reserve due to the necessity of future experiment, that they indicate two maxima in the heat curve, one corresponding within the limits of errors of observation to the solar curve maximum, the second indefinitely lower down in the spectrum, corresponding to a greater amount of heat at a lower temperature. Exactly what temperature this latter corresponds to, we have no present means of knowing. We have succeeded, however, in forming a measurable heat spectrum from the surface of a Leslie cube containing boiling water, and the maximum ordinate in the lunar heat curve appears to be below the maximum ordinate in the hot water curve. The inference from this is, of course, that the temperature of the lunar soil is, at any rate, below that of boiling water and in an indefinite degree.

We cannot close this note without calling attention to the remarkable fact that we here seem to have radiations from the moon of lower wave-length than from the sun, which implies an apparent contradiction to the almost universally accepted belief that the sun's emanations, like those from any heated solid body, include all low wave-lengths representing temperatures inferior to those certainly emitted.

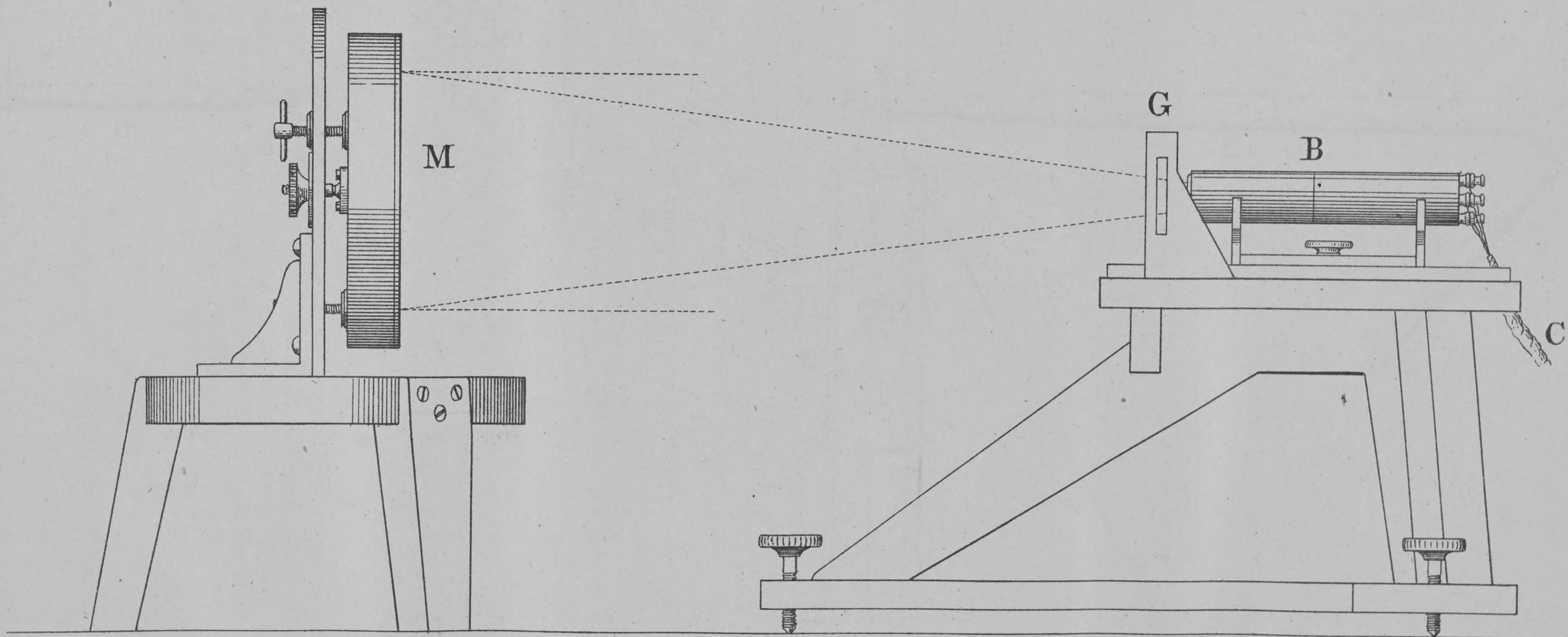


PLATE 1.—LUNAR HEAT APPARATUS.

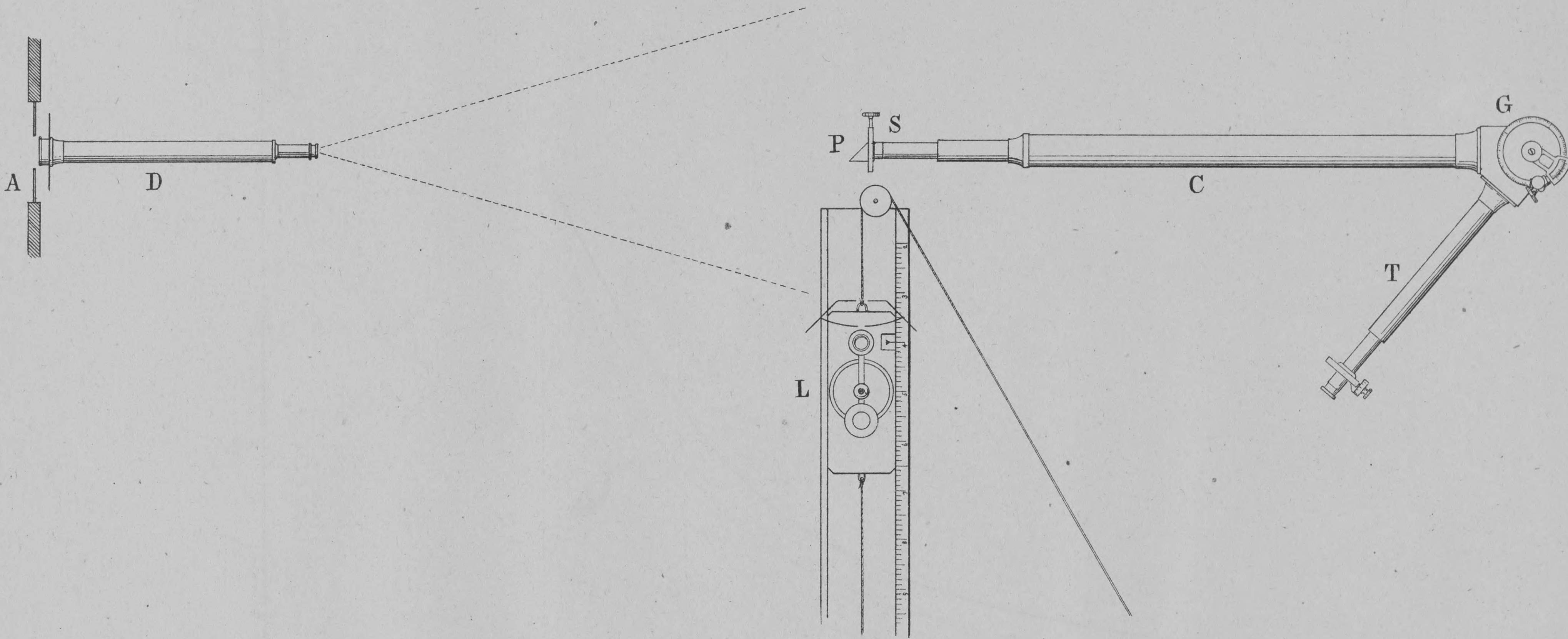


PLATE 2.—SPECTRO-PHOTOMETER.

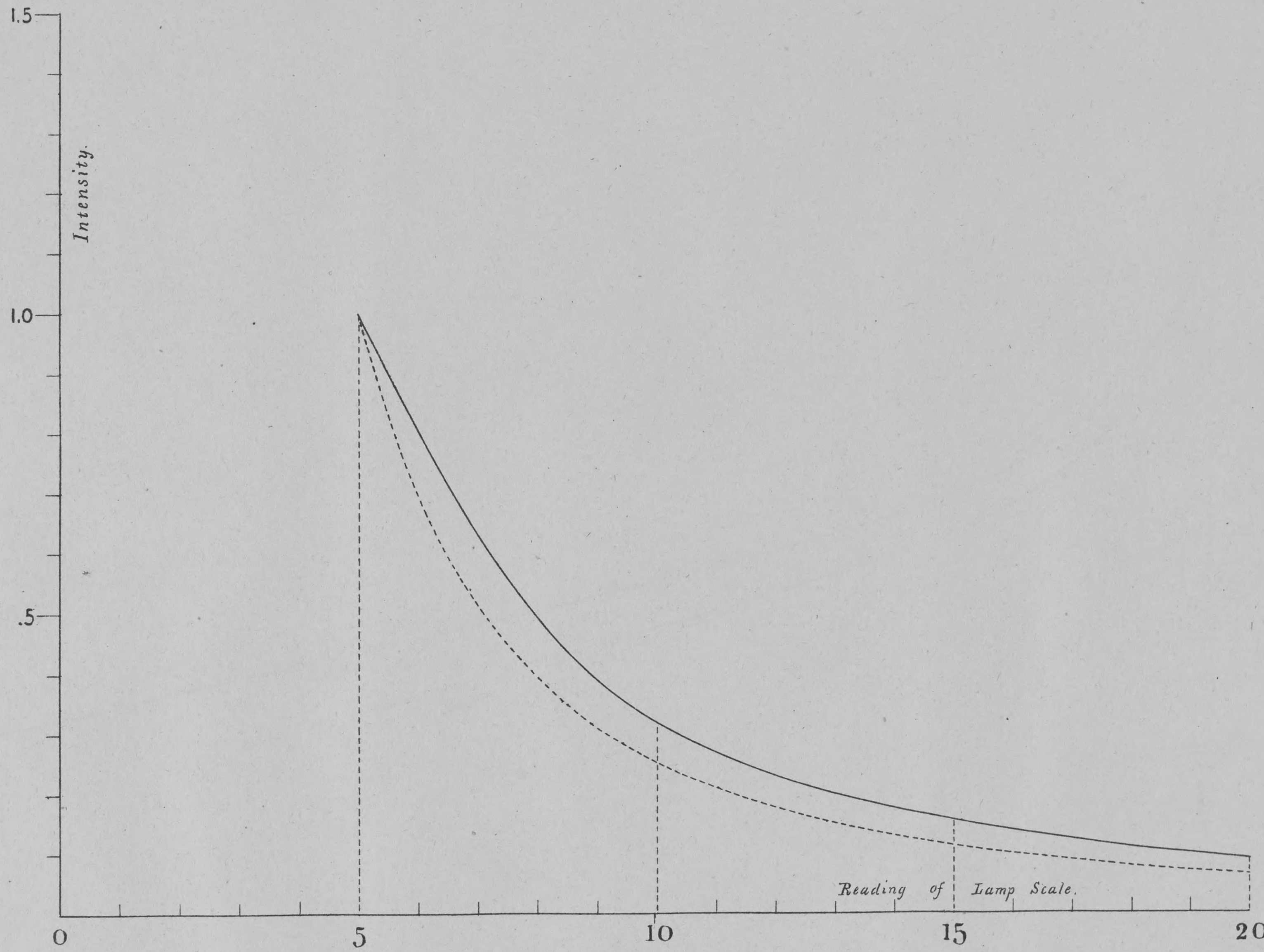


PLATE 3.—LIGHT-INTENSITY OF PHOTOMETER LAMP $= f$ (SCALE READING).

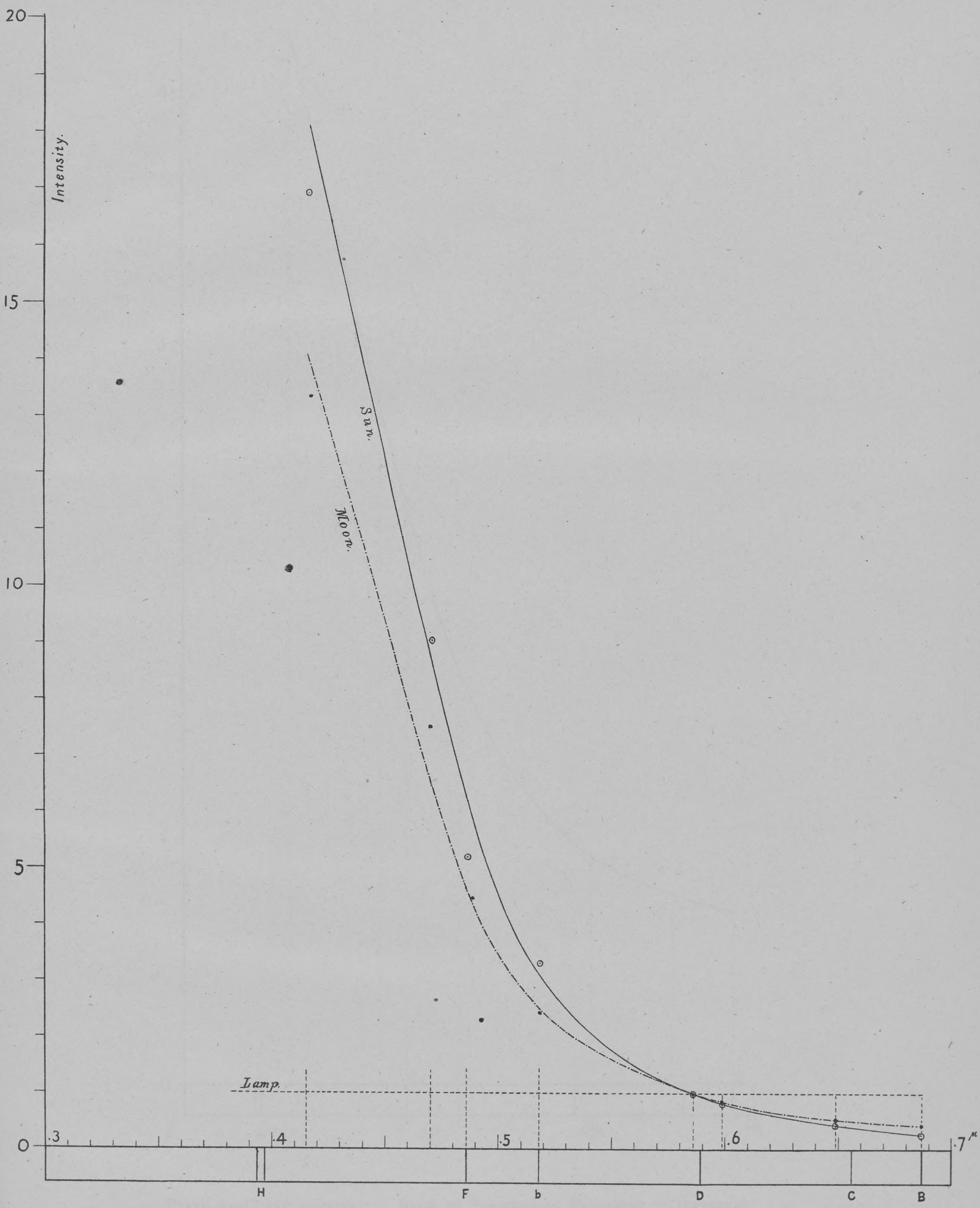


PLATE 4.—RELATIVE INTENSITIES OF SUNLIGHT, MOONLIGHT, AND LAMPLIGHT.

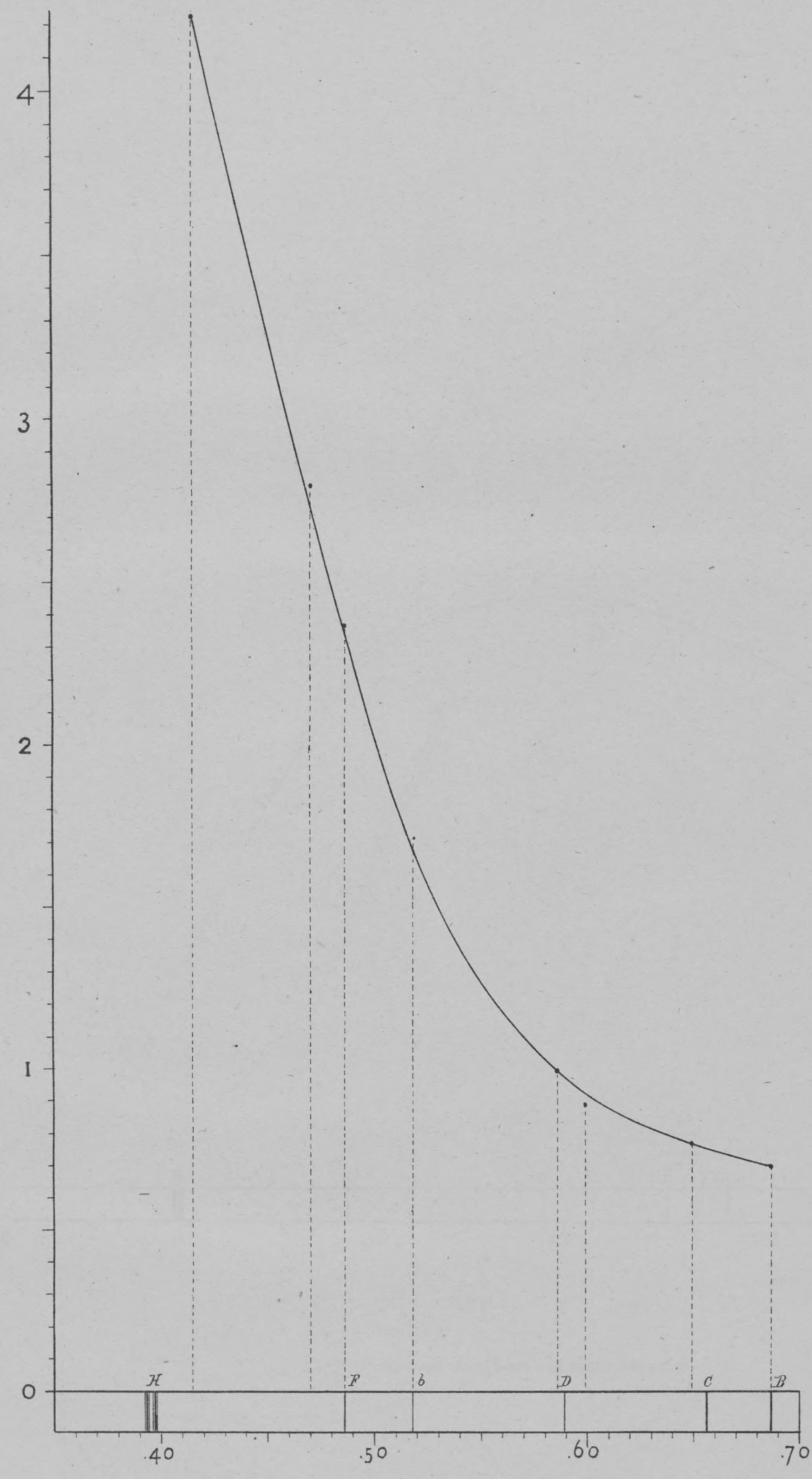


PLATE 5.—CURVE SHOWING THE RATIO OF SUNLIGHT TO MOONLIGHT IN DIFFERENT PARTS OF THE SPECTRUM.

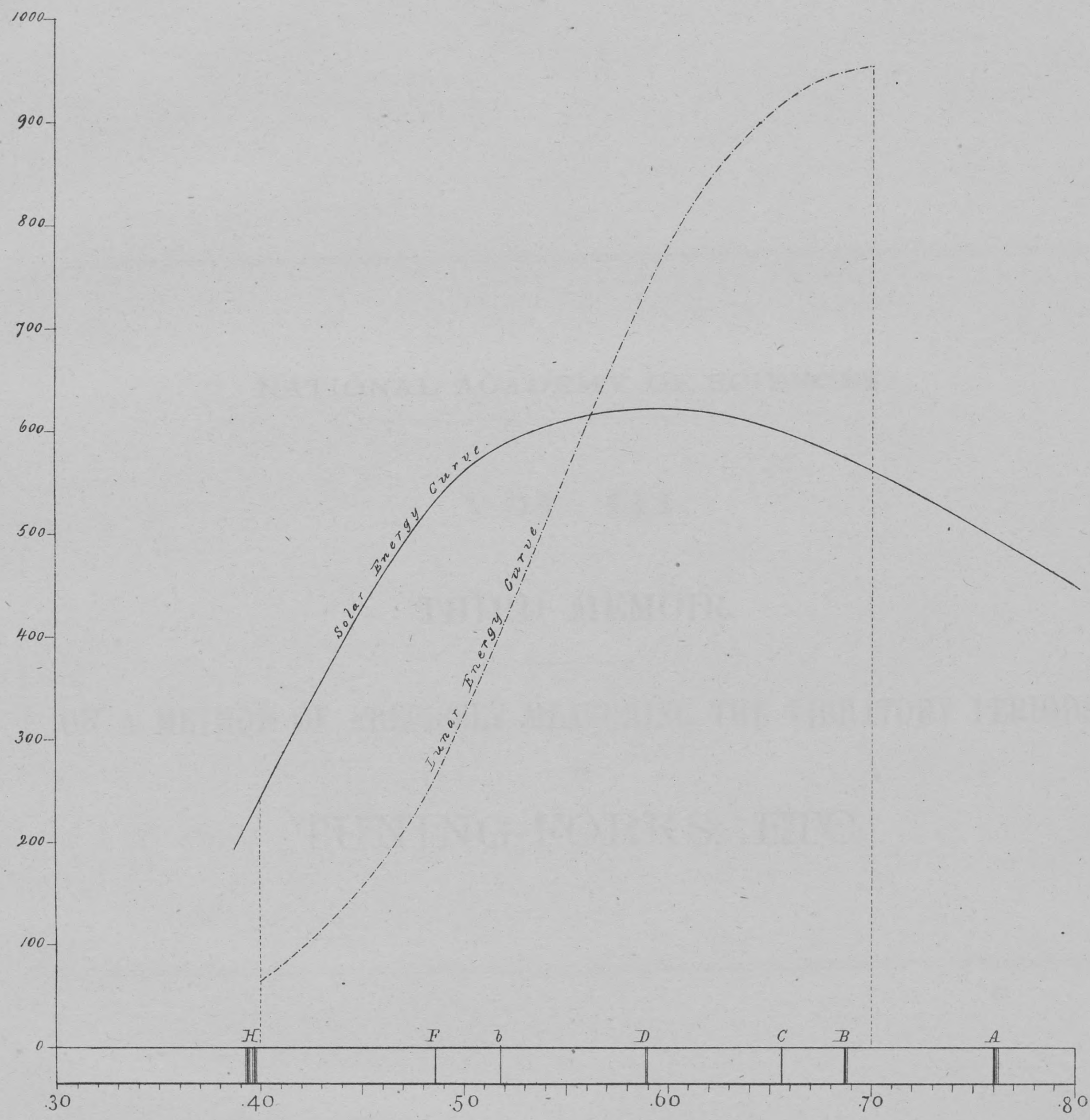
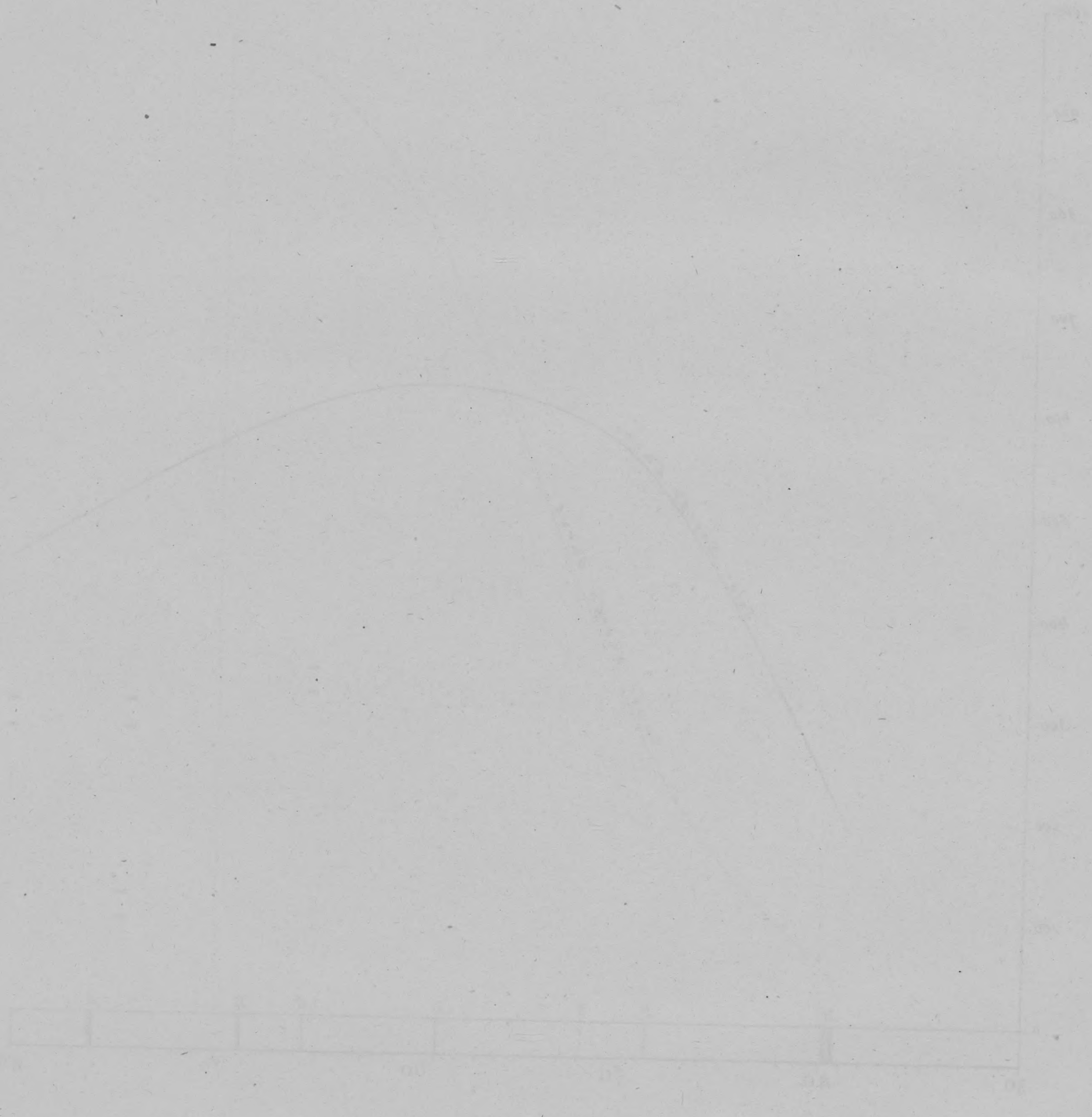


PLATE 6.—SOLAR AND LUNAR ENERGY CURVES.



NATIONAL ACADEMY OF SCIENCES.

VOL. III.

THIRD MEMOIR.

ON A METHOD OF PRECISELY MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS, ETC.

ON A METHOD OF PRECISELY MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS, AND THE DETERMINATION OF THE LAWS OF THE VIBRATIONS OF FORKS; WITH SPECIAL REFERENCE OF THESE FACTS AND LAWS TO THE ACTION OF A SIMPLE CHRONOSCOPE.

By Alfred M. Mayer.

This research was carried on with funds from the Bache endowment to the National Academy of Sciences. Its object was to arrive at a method of precisely measuring the vibratory periods of tuning-forks, and to determine the laws of the vibrations of forks, with the special reference of these facts and laws to the uses of the tuning-fork as a chronoscope in measuring small intervals of time.

The method devised is to make a clock, at each second, flash a spark of induced electricity on the trace made by a style attached to the prong of the vibrating fork *F*. *P*, Fig. 1, is the pendulum armed with a triangular piece of platinum foil, which, at each second, cuts through a globule of mercury contained in a small iron cup, *M*. This cup is so made that the globule can be regulated as to size and height by means of a screw-collar. Fresh mercury was placed in the cup at each experiment. The tuning-fork *F* is screwed into a board, *H*, which is hinged at *h*. This board rests against a screw-stop, *R*. *C* is a cylinder of brass, rotating on an axle, on one end of which is cut a screw, which runs in a nut at *T*. (See upper figure of Fig. 1.) The end of a prong of the fork is armed with a small triangle of thin elastic copper foil, about $\frac{1}{50}$ millimeter thick, and weighing only one milligram. The surface of the prong is well washed with ether, and then the foil is cemented to it with shellac. The point of this style just touches the camphor-smoked surface of paper, which tightly and smoothly envelops the cylinder *C*. The primary coil of an inductorium, *I*, and the clock (through *P*, and the globule of mercury, *M*) are placed in the circuit of a voltaic cell, *B*. In the secondary circuit of the inductorium is the fork *F* and the cylinder *C*, the thickness of the paper on the latter separating the point of the style on the fork from the surface of the brass cylinder. The fork is thrown upward, around the hinge *h*, vibrated by drawing a bow across a prong; then depressed till the board *H* comes against the stop *R*. The cylinder is rotated, and the trace of the fork is made on the paper, as shown in upper figure of Fig. 1. At each second, when the platinum-tipped pendulum *leaves* the globule of mercury, a spark flashes from the point of the style and makes *a single minute and circular* white spot on the blackened paper. This spot must be bisected by the trace of the fork. The center of the spot is generally marked by a minute perforation.

To obtain the results just described, it is necessary to fulfill certain conditions in the experiment, which, if neglected by experimenters, they would hastily regard the method as inaccurate. These conditions are as follows: (1) The globule of mercury should be small and *rigid;* that is, it should not vibrate when the platinum tip cuts through it. This condition is attained by screwing up the collar on the small cup *M* till only a small portion of the mercury is above the upper face of the cup. This adjustment has to be made with great care. The spark which at each second passes between the platinum tip and the mercury rapidly oxidizes the latter, and the mercury must be renewed at each experiment. (2) The paper on the cylinder must be smooth, thin, but not glazed. It required many days of experimenting before I succeeded in getting a paper which gave the results I sought. The best paper is a very thin printing paper with a smooth, unglazed surface. (3) The style on the fork must be very light and elastic. The best for this purpose is made out of thin hard-rolled copper or aluminium. (4) The spark given by the inductorium must be of the character already described. If the discharge of the inductorium be not composed of *a single spark*, and its impress on the paper a minute circular spot *bisected by the trace of the fork*, it will be useless to expect accurate results from this method.

To reach these conditions cost much time, and it may be interesting to describe some of the variations in the character of the discharge of an inductorium when excited by various strengths of current, and when condensers of various areas are or are not in the secondary circuit.

The flash of an inductorium appears composed of a single discharge; but only in certain conditions is it really composed of a single spark. If the discharge be obtained through the style on the fork with a current traversing the coil of a strength approaching that used in the usual electrical experiments with it, several flexures of the trace of the fork will be obliterated by the discharge deflagrating the carbon on the paper. This effect is produced by a multiplicity of discharges, following each other with such rapidity, and of such strength, as to denude the paper of carbon, to some extent, on either side of the trace. The breadth of carbon removed and the fuzzy character of the contour of these traces give them the appearance of caterpillars.

To obtain an analysis of these complex actions I devised the following method of experimenting, shown in Fig. 2: A revolving brass cylinder, similar to the one used in our apparatus just described, was covered with thin printing paper, and the latter was well blackened by rotating the cylinder over burning camphor. The paper was then removed from the cylinder and cut into disks of about 15 centimeters in diameter. When one of these disks is revolved about its center with a velocity of about 20 times per second, it is rendered very flat by centrifugal action. It can then be brought between points or balls, even when the latter are separated by no more than .75 millimeter. When in this position the discharge between the points or balls perforates the disk and leaves a permanent record of its character, of the duration of the whole discharge, and of the intervals separating its constituent flashes and sparks. To obtain the time of rotation I presented momentarily to the rotating disk a delicate point attached to the prong of a vibrating fork of known period of vibration. The axis of the sinuous trace thus made by the fork is traced by a needle point applied to the rotating paper disk. Drawing radii through symmetrical intersections of this axis on the sinuous line, we divide the disk off into known fractions of time. The disk is now removed from the rotating apparatus and the carbon is fixed by floating the disk for a moment on dilute spirit varnish. When the disk is dry it is centered on a divided circle provided with a low-power micrometer microscope, and the duration of the whole discharge and the intervals of time separating its components can be determined to the $\frac{1}{25000}$ of a second. I here give three typical experiments with this method, which will show the characteristics of the discharge of an inductorium:

1. DISCHARGE OF A LARGE INDUCTORIUM (STRIKING DISTANCE, 45 CENTIMETERS BETWEEN BRASS POINTS) BETWEEN PLATINUM POINTS 1 MILLIMETER APART. NO JAR OR CONDENSER IN SECONDARY CIRCUIT.

The platinum electrodes were neatly rounded and formed on wire $\frac{6}{10}$ millimeter in diameter. After the discharge through the rotating disk nothing was visible on it except a short arc formed of minute, thickly-set white dots; but on holding the disk between the eye and the light, it was found to be perforated with 33 clean, round holes, with the carbon undisturbed around their edges. The portion of the discharge which makes these holes lasts $\frac{1}{23}$ second, and the holes are separated by intervals which gradually decrease in size toward the end of the discharge, so that the last spark-holes are separated about one-half of the distance separating the holes made at the beginning of the discharge. The average interval between the spark-holes is $\frac{1}{759}$ second. After this portion of the discharge has passed there is a period of quiescence lasting about $\frac{1}{1500}$ second; then follows a shower of minute sparks, which forms the short dotted line already referred to. This spark-shower lasts $\frac{1}{330}$ of a second, and is formed of 30 sparks; hence the average interval separating these sparks is $\frac{1}{9900}$ second. The intervals separating these sparks are, however, not uniform, but are smaller in the middle of the spark-shower than at the beginning or at the end of this phenomenon. This spark-shower, indeed, is a miniature of the phenomenon obtained when a Leyden jar is placed in the secondary circuit of the coil, and which will be described in the following experiments. The above determinations of intervals of time in the discharge are the mean of measures on six disks.

2. DISCHARGE OF LARGE INDUCTORIUM BETWEEN PLATINUM POINTS ONE MILLIMETER APART, WITH A LEYDEN JAR OF 242 SQUARE CENTIMETERS SURFACE IN THE SECONDARY CIRCUIT OF THE COIL.

After this discharge through the disk a very remarkable appearance is presented. The discharge in its path around the rotating disk dissipates little circles of carbon. There are 91 of these circles, each perforated by 4, 3, 2, or 1 holes. I have to frame a new nomenclature to describe this complex phenomenon. I call the whole act of discharge of the coil, *the discharge.* Those separate actions which form the little circles by the dissipation of the carbon we will call *flashes*, and the perforations of these circles we call *spark-holes.* The discharge in the above experiment lasts $\frac{1}{24}$ second. The flashes at the beginning of the discharge are separated by intervals averaging $\frac{1}{555}$ second up to the tenth flash; after this the intervals of the flash rapidly close up, so that during the fourth fifth of the discharge they follow at each $\frac{1}{5880}$ of a second. During the last fifth of the discharge the intervals between the flashes gradually increase, and the last flash is separated from its predecessor by $\frac{1}{1000}$ of a second.

The appearance of the carbon-covered disk, after one of the discharges just described has passed through it, is given in Fig. 3.

On diminishing the current in the primary coil of the inductorium I found that the number of flashes in the discharge diminished, so that at last I obtained a discharge which consisted of but one flash perforated by one minute spark-hole. Also, if the current remain the same and a portion of the secondary circuit be divided, and gradually separated more and more, the number of flashes in the discharge will be diminished and the whole energy of the discharge concentrated in time. But no rule can be given for any special coil to obtain from it such a discharge as is alone useful in the work on the forks, and the current must be gradually varied by resistances in the primary circuit of the inductorium, and the area of the condenser in the secondary circuit, till the conditions for any special coil are obtained which cause it to give a spark which makes a minute circular and well-defined mark directly in the trace of the style of the fork. In the inductorium used there is 150 feet of wire in the primary circuit and eight miles in the secondary. The condenser in the secondary circuit was formed of tin-foil separated by panes of glass, and had an area of 50 square inches.

STUDY OF THE EFFECT OF VARYING AMPLITUDES OF VIBRATION OF THE FORK ON ITS VIBRATORY PERIOD; AND ON THE EFFECTS OF VARYING PRESSURES OF THE STYLE ON THE PAPER-COVERED CYLINDER.

The experiments on this fork of Kœnig's were made not so much for the determination of its vibratory period at a given temperature, as to discover any effect on the vibratory period caused by difference of amplitude of vibration, and by varying pressures of the tracing style on the smoked paper. This series of measures is given as an average example of series of similar sheets on which we have made measures. It will be observed that the vibration-numbers opposite the successive seconds, given in the first column, are alternately small and large. This is due to the fact that the center of the globule of mercury is not exactly on the vertical of the pendulum, but by taking the mean of two successive seconds we have the mean number of vibrations for those seconds. These means are given in column 3.

TABLE I.

(1)	(2)	(3)
1	255.00	255.95
2	256.90	
3	255.05	255.97
4	256.90	
5	254.90	255.90
6	256.90	
7	254.70	255.92
8	257.15	
9	254.95	256.02
10	257.10	
11	254.90	256.00
12	257.10	

(1) The mean of 1st and 2d seconds=255.95. Amplitude of vibration of 1st second = 2.03 millimeters.
The mean of 11th and 12th seconds = 256.00. Amplitude of vibration of 12th second = .63 millimeter.

From this observation one might conclude that the number of vibrations increased with a diminished amplitude, but the following observations show that this is not a just conclusion:

(2) Mean of 1st and 2d seconds=255.97. Amplitude of vibration of 1st second=1.19 millimeters.
Mean of 7th and 8th seconds=255.97. Amplitude of vibration of 8th second= .59 millimeter.
(3) Mean of 1st and 2d seconds=256.05. Amplitude of vibration of 1st second=2.39 millimeters.
Mean of 11th and 12th seconds=256.00. Amplitude of vibration of 12th second= .61 millimeter.
(4) Mean of 1st and 2d seconds=256.17. Amplitude of vibration of 1st second=2.07 millimeters.
Mean of 9th and 10th seconds=256.20. Amplitude of vibration of 10th second= .78 millimeter.

From the above measures we conclude that differences of amplitude of vibration in a fork, arranged as in the experiments, has no appreciable effect on its vibratory period.

Many measures were made on records obtained with varying pressures of the tracing style against the smoked paper; but the slight variations of those pressures which could be obtained within the range of elasticity of the delicate style used gave no differences in the number of vibrations from which we could detect any influence of varying pressures of the style.

EFFECT OF TEMPERATURE ON THE VIBRATORY PERIOD OF FORKS.

To determine the effect of variations of temperature on the vibratory periods of steel forks, I bought two sets of Kœnig's forks of the UT_2 harmonic series to known differences of temperature, and then determined how much they were thus thrown out of unison by the observation of the number of beats thus caused in one minute of time.

Instead of heating or cooling one set of the forks by automatic thermostats, which method had several objections in principle and great difficulties in the way of experimenting, I decided to wait for a favorable spell of weather, which we often have in April, when the air is still and misty and a drizzling rain occurs. In such weather the air is nearly constant in temperature. During such favorable conditions for the work, when the atmosphere varied only a few degrees in temperature during two days of mist and rain, I opened the windows of a room which contained one of the sets of forks and allowed them to remain there for a night and part of a day before beginning the experiments. In an adjoining room, kept at as nearly an equable temperature as possible, I placed the other forks. After the respective temperatures of these rooms had not varied perceptibly during three hours, I opened the door between the rooms just enough to hear clearly the forks of one room when stationed near the forks in the other. The temperature of the hot room was 66° Fahr., that of the other room was 41° Fahr.

Simultaneously sounding in order the two corresponding forks of the series, I obtained the following results. The beats were timed with the aid of a stop-watch registering to one-tenth of a second. After each observation the door was closed and fifteen minutes allowed to elapse before beginning the observations on the two forks next in order. I should here remark, however, that the order in which the forks were experimented with was the reverse of that given in the table, that is to say, the experiments began with the UT_5 fork, of highest pitch; because the smaller mass of the higher forks would be most affected by any change of temperature from interchange of air of the rooms. I, however, observed no change in the temperature of the rooms during the experiments.

TABLE II.

The two UT_2 forks gave 11.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_3 forks gave 23.0 beats in 60 seconds for a difference of 25° Fahr.
The two SOL_3 forks gave 26.0 beats in 60 seconds for a difference of 25° Fahr.
The two UT_4 forks gave 32.5 beats in 60 seconds for a difference of 25° Fahr.
The two MI_4 forks gave 67.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_5 forks gave 81.5 beats in 60 seconds for a difference of 25° Fahr.

The forks in the cold room were a set recently received of KŒNIG; those in the warm room were a set of his forks which had been in constant use for several years and had become worn and somewhat rusted. To ascertain the difference in the numbers of vibrations of corresponding forks of the two sets, when at the same temperature, I had kept them for a day in the room which had the temperature of 66° Fahr., and after they had remained at this temperature during four hours we simultaneously sounded the two corresponding forks of the two sets with the following results:

TABLE III.

New UT_2 fork gave	2.3 beats in 60 seconds, with old	UT_2 fork.	Old fork flat.
New UT_3 fork gave	5.0 beats in 60 seconds, with old	UT_3 fork.	Old fork flat.
New SOL_3 fork gave	2.0 beats in 60 seconds, with old	SOL_3 fork.	Old fork sharp.
New UT_4 fork gave	no beats in 60 seconds, with old	UT_4 fork.	Old fork in unison.
New MI_4 fork gave	12.0 beats in 60 seconds, with old	MI_4 fork.	Old fork flat.
New UT_5 fork gave	12.0 beats in 60 seconds, with old	UT_5 fork.	Old fork flat.

Correcting the observations of the number of beats given in Table II by the determination of beats contained in Table III, we have the actual numbers of beats per minute given by the forks for a difference in temperature of 25° Fahr., if the fork had been strictly in unison when at the same temperature, as follows:

TABLE IV.

The two UT_2 forks gave 9.3 beats in 60 seconds for a difference of 25° Fahr.
The two UT_3 forks gave 18.0 beats in 60 seconds for a difference of 25° Fahr.
The two SOL_3 forks gave 28.0 beats in 60 seconds for a difference of 25° Fahr.
The two UT_4 forks gave 34.5 beats in 60 seconds for a difference of 25° Fahr.
The two MI_4 forks gave 45.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_5 forks gave 69.6 beats in 60 seconds for a difference of 25° Fahr.

From the above determinations it follows:

TABLE V.

$-, +,$ 1° Fahr. gives	UT_2 fork $+, -,$	.00600 of a vibration per second.
$-, +,$ 1° Fahr. gives	UT_3 fork $+, -,$	.01200 of a vibration per second.
$-, +,$ 1° Fahr. gives	SOL_3 fork $+, -,$	.018666 of a vibration per second.
$-, +,$ 1° Fahr. gives	UT_4 fork $+, -,$	.023000 of a vibration per second.
$-, +,$ 1° Fahr. gives	MI_4 fork $+, -,$	.030400 of a vibration per second.
$-, +,$ 1° Fahr. gives	UT_5 fork $+, -,$	.046333 of a vibration per second.

The above results may be reduced to a more general statement by giving the effect of a change of 1° Fahr. on the forks' vibratory period, as follows:

TABLE VI.

+ 1° Fahr. diminishes UT_2 fork's vibratory period ($\frac{1}{128}$ second) $\frac{1}{21333}$ part.
+ 1° Fahr. diminishes UT_3 fork's vibratory period ($\frac{1}{256}$ second) $\frac{1}{21333}$ part.
+ 1° Fahr. diminishes SOL_3 fork's vibratory period ($\frac{1}{384}$ second) $\frac{1}{21333}$ part.
+ 1° Fahr. diminishes UT_4 fork's vibratory period ($\frac{1}{512}$ second) $\frac{1}{22217}$ part.
+ 1° Fahr. diminishes MI_4 fork's vibratory period ($\frac{1}{640}$ second) $\frac{1}{21052}$ part.
+ 1° Fahr. diminishes UT_5 fork's vibratory period ($\frac{1}{1024}$ second) $\frac{1}{22100}$ part.

From Table VI it is seen that the effect of a change of temperature on the vibratory period is the same for all forks made of the same steel and similarly shaped. The differences among the fractions of a vibratory period are small and evidently owing to the necessary errors of observation. I have great confidence in the accuracy of this determination. The mean fraction of the vibratory period which one of Kœnig's forks gains or loses by a diminution or increase of 1° Fahr. is $\frac{1}{21561}$ part, or .00004638.

THE LAW OF THE RUNNING DOWN IN THE AMPLITUDE OF A FORK'S VIBRATION.

Twelve sheets were carefully taken of the traces of an UT_2 fork of 128 vibrations per second. The fork was vibrated with a bow and the cylinder turned as uniformly as possible by the hand. The seconds were marked off on the traces of the fork by the break circuit of the clock. At or near each second mark on the sheets was measured with a microscope-micrometer the amplitude of the vibration. The whole number of the sheets furnished over two hundred measures, giving the connection between the time the fork had run and the amplitude of its vibration at the end of that time. A curve was then plotted giving their relations. Its discussion showed that it was a logarithmic curve, which has the following expression: $y=(1.119)^x$.

EFFECT OF THE SUPPORT OF A FORK AND OF THE SCRAPE OF ITS TRACING-STYLE ON ITS VIBRATORY PERIOD.

These experiments on the effects of the support and scrape of the fork were made in connection with Prof. Albert A. Michelson with special reference to the period of vibration of the fork he used in timing the rotation of the mirror he employed in his experiments on the velocity of light. The fork was an UT_3 of Kœnig.

TABLE VII.

No. 1.

Temp. 80° Fahr. 80—65=15
15×.012=.180=correction for temperature.

(1).... 0.3	(6)....1289.2	(10)....2303.5
(2).... 256.1	(7)....1535.3	(11)....2559.0
(3).... 511.7	(8)....1791.5	(12)....2825.3
(4).... 767.9	(9)....2047.1	(13)....3071.0
(5)....1023.5		

(7)—(1)÷6=255.83
(8)—(2)÷6=255.90
(9)—(3)÷6=255.90
(10)—(4)÷6=255.93
(11)—(5)÷6=255.92
(12)—(6)÷6=256.01
(13)—(7)÷6=255.95

Mean............255.920
Corr. for temp.... +.180

256.100
Corr. for clock... —.028

256.072

No. 2.

Temp. 81° Fahr. 81—65=16
16×.012=.192

(1).... 0.6	(5)....1024.5	(9)....2048.6
(2).... 256.7	(6)....1280.6	(10)....2304.9
(3).... 512.4	(7)....1536.2	(11)....2560.2
(4).... 768.3	(8)....1792.3	

(6)—(2)÷4=255.97
(7)—(1)÷6=256.93
(8)—(2)÷6=255.93
(9)—(3)÷6=256.03
(10)—(4)÷6=256.10
[(11)—(5)÷6=255.95]?

Mean 255.962
Corr. for temp.... +.192

256.154
Corr. for clock.... —.028

256.126

TABLE VII—Contineud.

No. 3.

Temp. 81° Fahr. 81—65=16 16×.012=.192

(1).... 0.1	(6)..........	(10)....2302.1
(2).... 254.5	(7)....1535.2	(11)..........
(3)..........	(8)....1790.3	(12)....2814.0
(4).... 766.3	(9)....2047.0	(13)....3071.1
(5)....1023.5		

(7)—(1)÷6=255.85
(8)—(2)÷6=255.97
(9)—(1)÷8=255.86
(10)—(4)÷6=255.97
(12)—(4)÷8=255.96
(13)—(5)÷8=255.95

Mean............255.927
Corr. for temp.... +.192

256.119
Corr. for clock.... —.028

256.091

No. 4.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1).... .03	(6)..........	(10)....2307.2
(2).... 258.4	(7)....1536.1	(11)....2560.0
(3).... 512.1	(8)....1795.0	(12)....2819.3
(4).... 771.1	(9)....2048.1	(13)....3072.3
(5)....1024.1		

(7)—(1)÷6=255.97
(8)—(2)÷6=256.10
(9)—(3)÷6=256.00
(10)—(4)÷6=256.02
(11)—(5)÷6=255.97
(12)—(4)÷8=256.02
(13)—(7)÷6=256.03

Mean............256.016
Corr. for temp.... +.120

256.136
Corr. for clock ... —.028

256.108

No. 5.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1).... 0.5	(5)....1024.3	(9)....2048.2
(2)....253.6	(6)..........	(10)....2301.3
(3)....512.3	(7)....1536.5	(11)....2560.0
(4)....765.5	(8)....1789.5	

(7)—(1)÷6=256.00
(8)—(2)÷6=255.98
(9)—(3)÷6=255.98
(10)—(4)÷6=255.97
(11)—(5)÷3=255.95

Mean............255.976
Corr. for temp .. +.120

256.096
Corr. for clock... —.028

256.068

No. 6.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1).... 0.7	(5)....1024.7	(9)....2048.7
(2)....258.6	(6)....1282.7	(10)....2306.6
(3)....512.7	(7)....1536.6	(11)....2560.6
(4)....770.5	(8)....1794.5	(12)....2818.9

(7)—(1)÷6=255.98
(8)—(2)÷6=255.98
(9)—(3)÷6=256.00
(10)—(4)÷6=256.02
(11)—(5)÷6=255.98
(12)—(6)÷6=256.03

Mean............255.998
Corr. for temp... +.120

256.118
Corr. for clock ... —.028

256.090

No. 7.

Temp. 75° Fahr. 75—65=10 10×.012=120

(1).... 0.1	(5)....1023.7	(9)....2047.5
(2).... 257.9	(6)....1281.6	(10)....2306.2
(3).... 512.0	(7)....1536.0	(11)....2560.0
(4).... 770.0	(8)....1794.0	(12)....2818.3

(7)—(1)÷6=255.98
(8)—(2)÷6=256.02
(9)—(3)÷6=255.92
(10)—(4)÷6=256.05
(11)—(5)÷6=256.05
(12)—(6)÷6=256.12

Mean............256.020
Corr. for temp... +.120

256.140
Corr. for clock... —.028

256.112

No. 8.

Temp. 76° Fahr. 76—65=11 11×.012=132

(1).... 0.0	(5)....1023.7	(9)....2048.1
(2).... 254.1	(6)....1278.1	(10)..........
(3).... 512.0	(7)....1536.2	(11)....2559.9
(4).... 766.0	(8)....1790.1	

(7)—(1)÷6=256.03
(8)—(2)÷6=256.00
(9)—(3)÷6=256.02
(8)—(4)÷4=256.02
(11)—(5)÷6=256.03

Mean...........256.020
Corr. for temp... +.132

256.152
Corr. for clock... —.028

256.124

TABLE VII—Continued.

No. 9.

Temp. 81° Fahr. 81—65=16 16×.012=.192

(1).... 0.8	(5)....1024.3	(9)....2048.4
(2).... 257.2	(6)....1280.7	(10)....2304.0
(3).... 512.7	(7)....1536.3	(11)....2560.2
(4).... 768.9	(8)....1792.5	

(7)—(1)÷6=255.92
(8)—(2)÷6=255.88
(9)—(3)÷6=255.95
(10)—(4)÷6—255.85
(11)—(5)÷6=255.98

Mean 255.916
Corr. for temp ... +.192

256.108
Corr. for clock... —.028

256.080

No. 10.

Temp. 81° Fahr. 81—65=16 16×.012=.192

(1).... 0.6	(6)....1281.9	(10)....2305.7
(2).... 258.1	(7)....1535.9	(11)....2559.7
(3).... 512.6	(8)....1793.8	(12)....2817.3
(4).... 770.0	(9)....2047.5	(13)....3071.5
(5)....1024.1		

(7)—(1)÷ 6=255.88
(8)—(2)÷ 6=255.95
(9)—(3)÷ 6=255.82
(10)—(4)÷ 6=255.95
(11)—(5)÷ 6=255.93
(12)—(6)÷ 6=255.90
(13)—(1)÷12=255.91

Mean 255.906
Corr. for temp.... +.192

256.098
Corr. for clock.... —.028

256.070

The mean value of the above-determined ten means is as follows:

(1)......256.072
(2)......256.126
(3)......256.091
(4)......256.108
(5)......256.068
(6)......256.090
(7)......256.112
(8)......256.124
(9)......256.080
(10)......256.070

256.094
Correction for effects of support and scrape. =—.026

256.068 Number of vibrations of fork on resonant box at 65° Fahr.

The correction —.026 for the effect of support and scrape of style of fork was determined as follows:

The standard UT_3 fork was placed in the same support (*H* of Fig. 1) which held it while it made its record on smoked paper, but fork vibrated freely, that is, it did not trace its vibrations on the paper. Another similar UT_3 fork was screwed on its resonant box and its prongs loaded with wax till it made about five beats per second with first fork. The beats were counted by coincidences with the one-fifth second beats of a watch.

TABLE VIII.

Coincidences were marked at 32 seconds; 39 seconds; 43.5 seconds; 49 seconds; 54.5 seconds; 61.5 seconds.
61.5—32=29.5; 29.5÷5=5.9=time of one interval between coincidences.

RÉSUMÉ.—(1)=5.9 seconds; (2)=6.2 seconds; (3)=6.2 seconds; (4)=6.2 seconds. Mean=6.13=time of one interval between coincidences.

In this time, the watch makes 6.13×5=30.65 beats, and the forks make 30.65+1=31.65 beats. Hence the number of beats per second is 31.65÷6.13=5.163.

We now made similar experiments to the above, with the difference that the standard UT_3 fork was allowed to make its trace on the smoked paper, as it did when we determined its rate of vibration.

TABLE IX.

Coincidences were marked at 59 seconds; at 4 seconds; at 10.5 seconds; at 17 seconds.

77—59=18; 18÷3=6.0=time of one interval.

RÉSUMÉ.—(1)=6.0 seconds; (2)=6.0 seconds; (3)=6.7 seconds; (4)=6.3 seconds; (5)=6.5 seconds; (6)=6.7 seconds; (7)=6.0; mean=6.31 seconds.

$$6.31\times5=31.55 \qquad 31.55+1.00=32.55$$

$$\begin{array}{rr} 32.55\div6.31= & 5.159 \\ \text{With fork free}= & 5.163 \\ \hline \text{Effect of scrape}= & -.004 \end{array}$$

Circumstances as in first case, except that both forks were on their resonant boxes.

TABLE X.

Coincidences were observed at 21 seconds; at 28 seconds; at 36 seconds; at 44 seconds; at 51 seconds; at 60 seconds.

60—21=39; 39÷5=7.8=time of one interval.

RÉSUMÉ.—(1)=7.8 seconds; (2)=7.1 seconds; (3)=7.6 seconds; (4)=7.4 seconds; (5)=7.2 seconds; mean=7.42 seconds.

$$\begin{array}{rr} 72.42\times5= & 37.10 \\ + & 1.00 \\ \hline & 38.10 \\ \hline\hline 38.10\div7.42= & 5.133 \\ \text{Above}= & 5.159 \\ \hline \text{Effect of support and scrape}= & -.026 \end{array}$$

From the experiments it appears that the effect of the work of the fork in tracing its record on the smoked paper covering the cylinder, is only —.004 of a vibration; a quantity so small as to be negligible, as will appear further on where we give the probable error of the mean determination of the numbers of vibrations per second of various forks.

The difference in the number of vibrations given by the fork when vibrating on its resonant box and when vibrating while screwed into the hard wooden support (H, Fig. 1), amounts to —.026 less .004, or —.022. This result was not anticipated, and it shows how careful should experimenters be in describing minutely the character of the support of the fork when they give the value of its vibratory period.

Determination of the numbers of vibration per second of European forks of various standards of pitch

[Sent me by Mr. Alexander J. Ellis, F. R. S.]

These forks were the A fork of 1789, of the Chapelle Versailles; the A fork of 1812, of the Conservatoire; the A fork of 1818, of the Theâtre Feydeau; the A fork of 1820, of the Tuilleries, and a C fork made by Marloye of Paris.

The determination of the pitch of these forks was made with special care, and these measures may be regarded as the limits of accuracy of our method, so far as I have been able to deal with it. The fractions of vibrations on the records were read off with a microscope-micrometer, and the corrections for temperature and rate of clock were carefully obtained.

TABLE XI.

[Chapelle-Versailles (*A*) fork of 1789.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction +.0185 (*t*−65).	Vibrations in one second of mean time at 65° F.
		° *F.*						
1	1	65.3	+5.2	.99721	394.495	395.599	+.005	395.604
	3	65.3	+5.2	.99721	394.543	395.647	+.005	395.652
	4	65.3	+5.2	.99721	394.536	395.640	+.005	395.645
2	1	65.0	+5.2	.99721	394.509	395.613	+.000	395.613
	2	65.0	+5.2	.99721	394.470	395.574	+.000	395.574
	3	65.0	+5.2	.99721	394.570	395.674	+.000	395.674
	4	65.0	+5.2	.99721	394.493	395.597	+.000	395.597
	5	65.0	+5.2	.99721	394.517	395.621	+.000	395.621
3	1	65.0	+5.2	.99721	394.512	395.616	+.000	395.616
	2	65.0	+5.2	.99721	394.502	395.606	+.000	395.606
	3	65.0	+5.2	.99721	394.507	395.611	+.000	395.611
	4	65.0	+5.2	.99721	394.470	395.574	+.000	395.574
	5	65.0	+5.2	.99721	394.495	395.599	+.000	395.599
								395.614

TABLE XII.

[Conservatoire (*A*) fork of 1812.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction +.020 (*t*−65).	Vibrations in one second of mean time at 65° F.
		° *F.*						
1	1	65	+5.30	.99721	438.128	439.354	.000	439.354
	2	65	+5.30	.99721	438.075	439.301	.000	439.301
2	1	64.75	+5.30	.99721	438.161	439.387	−.005	439.382
3	1	63.6	+5.30	.99721	438.134	439.360	−.028	439.332
4	1	63.6	+5.30	.99721	438.118	439.344	−.028	439.316
								439.337

TABLE XIII.

[Théâtre Feydeau (*A*) fork of 1818.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction +.019 (*t*−65).	Vibrations in one second of mean time at 65° F.
		° *F.*						
1	1	63.4	+5.2	.99721	432.812	434.023	−.031	433.922
	2	63.4	+5.2	.99721	432.799	434.010	−.031	433.979
	3	63.4	+5.2	.99721	432.809	434.019	−.031	433.988
	4	63.4	+5.2	.99721	432.800	434.011	−.031	433.980
2	1	63.8	+5.2	.99721	432.847	434.058	−.023	434.035
	2	63.8	+5.2	.99721	432.818	434.029	−.023	434.006
	3	63.8	+5.2	.99721	432.844	434.055	−.023	434.032
	4	63.8	+5.2	.99721	432.825	434.036	−.023	434.013
3	1	64.5	+5.2	.99721	432.799	434 010	−.010	434.000
	2	64.5	+5.2	.99721	432.837	434.048	−.010	434.038
	3	64.5	+5.2	.99721	432.805	434.016	−.010	434.006
	4	64.5	+5.2	.99721	432.828	434.039	−.010	434.027
								434.008

TABLE XIV.

[Tuilleries (A) fork of 1820.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction + .019 (t—65).	Vibrations in one second of mean time at 65° F.
		° F.						
1	1	65.5	+4.9	.99721	421.589	422.769	—.01	422.759
	2	65.5	+4.9	.99721	421.597	422.776	—.01	422.766
	3	65.5	+4.9	.99721	421.626	422.806	—.01	422.796
2	1	65.8	+4.9	.99721	421.632	422.812	—.015	422.797
	2	65.8	+4.9	.99721	421.636	422.816	—.015	422.801
	3	65.8	+4.9	.99721	421.641	422.821	—.015	422.806
3	1	65.5	+4.9	.99721	421.667	422.847	—.01	422.837
	2	65.5	+4.9	.99721	421.609	422.788	—.01	422.778
	3	65.5	+4.9	.99721	421.624	422.804	—.01	422.794
								422.793

TABLE XV.

[Marloye (C) fork.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction.	Vibrations in one second mean time at 65° F.
		° F.						
3	1	69.25	+3.48	.99723	255.167	255.876	+.051	255.927
4	1	69.5	+3.48	.99723	255.224	255.933	+.054	255.987
6	1	71	+3.48	.99723	255.187	255.896	+.072	255.968
	2	71	+3.48	.99723	255.120	255.829	+.072	255.901
7	1	60	+4.35	.99722	255.208	255.919	—.057	255.862
	2	60	+4.35	.99722	255.219	255.930	—.057	255.873
8	1	61	+5.00	.99721	255.212	255.926	—.048	255.878
	2	61	+5.00	.99721	255.221	255.935	—.048	255.887
								255.910

The determinations of the number of vibrations per second of the A forks have to be corrected by +.044 for the effect of the weight of the tracing style. The correction was too small to be determined in the case of the C fork.

The separate determinations of the number of vibrations of the Chapelle Versailles fork and those of the Théâtre Feydeau fork are numerous enough to give some idea of the probable error of a single determination and of the error of the mean of the determinations when these are discussed by the method of least squares.

From this discussion it appears that for the Chapelle Versailles fork, the probable error of a single determination= ±.019 of a vibration; the probable error of the mean determination= ±.0053 of a vibration.

From the experiments on the Théâtre Feydeau fork, the probable error of a single determination= ±.014 of a vibration; the probable error of the mean determination= ±.004 of a vibration.

These results show that the method is quite accurate, and certainly sufficiently so for the determination of the pitch of a standard fork, and for all purposes when the fork is used as a chronoscope in the measure of small intervals of time. If the error of the determination of the pitch of these two forks—when corrected for effects of support and scrape, which is a constant readily determined—should only equal $\frac{5}{1000}$, or $\frac{1}{200}$ of a vibration in one second, a variation of that amount would be produced by a change of temperature of only one-fourth of a degree F. in the Théâtre Feydeau fork, and if measured in beats would amount to the difference in the pitch of two forks, which, when sounded together, would give one beat in 200 seconds.

ON THE USES OF THE TUNING-FORK AS A CHRONOSCOPE.—Various forms of chronoscopic apparatus contain a vibrating fork as a register of time. The majority of these are costly, by reason of the attempts of the inventors to obtain regular rotations of cylinders or disks by means of clock-work, when really all such appliances are useless. The fork itself, if only allowed to register its own trace on the revolving cylinder or disk, will give all that is desired without such adjuncts, for the accuracy of its registration has no connection with the rotation of the cylinder on which it leaves its record, and it matters not whether the latter be revolved quicker or slower, regularly or irregularly, so long as the motion is appreciably uniform during the trace of one flexure of the fork; this duration in the case of an UT_3 fork would be only the $\frac{1}{256}$ of a second, and in that minute interval it would not be possible to get a measurable variation in velocity unless we did our best to attain it. Any ordinary care in the rotation of the cylinder by hand will give waves which at and near the spark-mark will be found to be similar and equal, and therefore no error can be made in the measure of the fraction of a wave.

The numbers of vibrations of a fork per second can be determined to $\frac{1}{200}$ of a vibration, or, to be surely within bounds, say to the $\frac{1}{100}$ of a vibration, by the method we have described in this paper. This will give the time record with an A fork of 440 vibrations per second to $\frac{1}{44000}$ of a second.

It is not necessary to make any correction for the effect of the scrape or weight of tracing style or for the effect of the kind of support of the fork, for the number of the fork's vibrations per second is determined while the fork is on the same support it has when used as a chronoscope and while the fork is making its record; in other words, the number of the fork's vibrations per second are determined *in the exact conditions in which it is used as a chronoscope.*

The arrangement of such a chronoscope is of the simplest character. Fig. 4 shows it. As an example, we will suppose that we are to determine the initial velocity of a rifle-ball. *B* is a voltaic cell, whose current goes through the primary coil of the inductorium *I*, then to the target *T* formed of a metal plate (or a screen of wire, if we are determining the velocity of a cannon ball). This plate is very slightly inclined forwards, so that its upper edge presses very slightly against an adjusting screw at *S*. The abutting surfaces of this screw and the plate are amalgamated to insure good elastic contact. The bottom of the plate rests in a small trough of mercury. The current passes to this trough and out of the plate at the adjusting screw *S*, thence to the make-circuit lever *MC*, and back to the battery *B*. One pole of the secondary circuit of the inductorium is connected with the fork *F*, the other pole with the rotating cylinder *C*. The make-circuit lever is formed in this manner: It moves around a center at *O*. On its lower side are two platinum lugs. By the motion of the lever around *O*, either one or the other of these lugs are brought in contact with two platinum contact-pieces, *e* and *c*, which are insulated from the plate and standard on which the lever is supported.

The chronoscopic apparatus having been arranged as in the diagram, the fork is raised on the hinge *h* (see Fig. 1) and vibrated with a bow. The cylinder is revolved and the fork brought down on its smoked-paper surface. At the word "fire," the rifle is discharged. The fine wire or thread *w* is cut by the ball, and the weight *p* which it supported and which brought the left hand platinum lug onto the left hand insulated contact-piece, falls; then the spring *s* (or, better, a rubber band), which opposed the action of the weight, swings the right hand lug on to the right hand contact-piece. When the ball cut the wire, the primary circuit of the inductorium was broken, and a spark, at that instant, passed from the style of the fork and made a spark-hole in its sinuous trace. But the spring *s* at once made contact again, and the circuit was made through the right-hand lug *c*. The ball, therefore, reaches the target-plate *T* with the circuit closed, and when it strikes *T* the plate is thrown from the contact-screw *S*, and a second break takes place in the primary circuit and another spark passes from the style of the fork. By counting the number of waves and measuring with a microscope-micrometer the fraction of the wave in the trace of the fork, we have the time it took the ball to go over the known distance from the wire w to the target T.

As an example of such work, we here give experiments we made on the velocity of the rifle-ball of .45 inch caliber of the United States Army cartridge. This ball weighs 405 grains, and the powder driving it weighs 70 grains.

TABLE XVI.

Number of experiment.	Waves between spark-holes.	Time in going over 60 feet.	Velocities per second.	Differences.
		Seconds.	*Feet.*	*Feet.*
(1)	11.31	.04418	1,358.0	+0.7
(2)	11.34	.04429	1,354.7	—2.6
(3)	11.30	:04414	1,359.3	+2.0
(4)	11.28	.04406	1,361.7	+4.4
(5)	11.35	.04433	1,353.3	—4.0
(6)	11.32	.04421	1,357.1	—0.2
			1,357.3	

The fifth column gives the differences of the separate determinations, and 1,357.3 feet the mean velocity of the ball per second. The average difference amounts to only 2.3 feet.

EXPERIMENTS WITH THE CHRONOSCOPE ON THE VELOCITIES OF FOWLING-PIECE SHOT OF VARIOUS SIZES PROJECTED WITH VARIOUS CHARGES OF POWDER FROM 12 AND 10 GAUGE GUNS.

The guns used in these experiments were "choke-bore," of the Colt Arms Manufacturing Company, of Hartford, Conn. They had rebounding locks. The primary current of the inductorium passed through a break-piece fixed under the rebounding hammer, so that at the instant the cartridge was exploded the electric current in the primary circuit of the inductorium was broken and then immediately formed again. The current which passed through this break-piece was led by a wire to an upright piece of tin plate, whose front surface leaned against a thick copper wire. Another wire led from the tin plate (which stood in a shallow trough of mercury) back to the battery.

The following tables give the results of our experiments:

TABLE XVII.

[10-gauge Colt gun; 5 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 1 Buck	1153	1067	....
FF	1147	1132	....
BB	1146	1126	....
No. 3	1066	1015	928
No. 6	1012	963	859
No. 8	995	880	775
No. 10	908	803	716

TABLE XVIII.

[10-gauge Colt gun; 4 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 1 Buck	1067	1018	....
FF................	1017	1009	967
BB................	1000	967	897
No. 3	989	911	872
No. 6	966	883	806
No. 8	920	874	776
No. 10	848	756	669

TABLE XIX.

[12-gauge Colt gun; 3¼ drams of Curtis & Harvey powder; 1⅛-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
BB................	862	795	667
No. 3..............	844	754	696
No. 6..............	825	739	600
No. 8..............	816	749	607
No. 10.............	796	680	610

TABLE XX.

[12-gauge Colt gun; 4 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 8..............	847	722	671
No. 10.............	748	657	596

Each measure of velocity given in these tables is the mean value obtained from several experiments, varying in number from three to six. The headings "velocity 30, 40, 50 yards," mean that the numbers under them give the average velocities of the flight of shot over those distances, and not the velocities at 30, 40, and 50 yards from the gun.

It will be observed that the shot used were Nos. 10, 8, 6, 3, BB, FF, and No. 1 Buck. They were so selected because a pellet of any number in the above series weighs nearly double the preceding one. Thus a pellet of No. 8 weighs double one of No. 10, a pellet of No. 6 weighs double one of No. 8, and so on. These relations of weight among the pellets were obtained so that I could readily reach the relations existing between the velocities and the weights of pellets. The shot used was kindly furnished me by Tatham & Bros., of New York, who used carefully gauged sieves in their manufacture. The powder used was Curtis & Harvey's Diamond Grain No. 6. The powder and shot in each cartridge had been carefully weighed out in an accurate balance.

A glance at the tables at once shows the rapid increase in the velocity of the shot from No. 10 up to No. 3. With the heavier pellets the increase is less marked. Thus the table headed "10 Colt gun; 4 drams, Curtis & Harvey, 1¼ shot," shows that No. 8 shot has 72 feet per second velocity over No. 10 shot, and No. 6 has 46 feet over No. 8, while No. 3 has only 23 feet over No. 6, and BB shot gains only 11 feet over No. 3.

The relations between velocity and weight of pellet shown in this table may be taken as a type of all the experiments, and I have graphically shown their relations in the accompanying curve.

The divisions on the scale, measured on the axis of ordinates, give the velocity per second of the pellets. One unit on this axis equals 20 feet, and a unit on the axis of abscissas equals one unit of weight of pellet. The weight of a pellet of No. 10 shot is here taken as the unit of weight. The numbers of the shot are written under the axis of abscissas, the velocities along the axis of ordinates.

My friend Professor Rice, of the United States Naval Academy, who had previously made similar experiments with a Le Boulengé chronoscope, and who took great interest in these experiments, found that the curve here given is very nearly the curve of secants, and the formula for it is:

$$\frac{y}{b} = \sec.^{-1} \frac{x^n}{a}$$

where x is the velocity and y the weight of a pellet, and a b and n undetermined constants.

So far as the experiments with these two special guns show, there is a marked superiority in the 10 over the 12 gauge, when each is loaded with the same weight of powder and shot. Thus, with the same charges, viz, 4 drams powder and 1¼ ounces of shot fired from the 10 gauge, gives a velocity of 100 feet per second more than that given by the 12-gauge gun. This fact is conclusively shown in the comparison of the figures in the two tables XVIII and XX, and the difference in velocities is in favor of the 10 gauge in each of the sixty experiments which were made to get the numbers contained in the lines opposite No. 8 and No. 10.

With No. 10 shot the mean velocity given by the 10-gauge gun over the first 30 yards is 848 feet. With the same charge in the 12 gauge the velocity is 748 feet; showing a difference of 100 feet in favor of the 10 gauge. With No. 8 shot the experiments show a difference of 72 feet. The average difference in favor of the 10 gauge in the flight of shot Nos. 8 and 10 over 40 yards amounts to 110 feet.

If we assume, as we may without grave error, that the penetration of shot varies as the square of its velocity, these experiments will give the relative penetrations of the 10 to the 12 gauge gun about as 9 is to 7.

That the 10-gauge gun shows such marked superiority over the 12 may be accounted for by the fact that the same charge occupies less length in a 10 than in a 12-gauge, and hence there are fewer pellets in contact with the barrel of the former than of the latter to oppose by their friction the projectile force of the powder. Also, as these choke-bores are contracted two sizes at their muzzles, the action of the choke on the pellets in a 10-gauge, will, I think, be more effective than in the case of a 12, the pellets in the latter being more crowded together and conflicting in their actions than in the case of their discharge from a 10 bore. Also, some effect in favor of the 10-gauge may be owing to the fact that in this gun the powder is exploded nearer the center of the charge, and thus there is less chance of it blasting before it unburnt powder contained in the portion of the charge removed from the point of ignition.

I also venture to predict that with the same weight of barrels the 10-gauge will not heat as much as the 12, because the motion of the shot lost in the 12-gauge must appear in the form of heat.

The simplicity and inexpensiveness of the chronoscope we have described in this paper, its accuracy, and the ease with which it is used must commend it to all who will give it a trial under the conditions of its action which we have endeavored to set forth in this paper. Another of its advantages is that its records on the paper covering the cylinder are easily rendered permanent by drawing the unsmoked side of the paper over the surface of a dilute solution of photographic negative varnish contained in a wide shallow dish. On the records may be written with a blunt style the nature of the experiments they record before the carbon is fixed by the varnish, and then they can be bound togetner in book-form for preservation and reference.

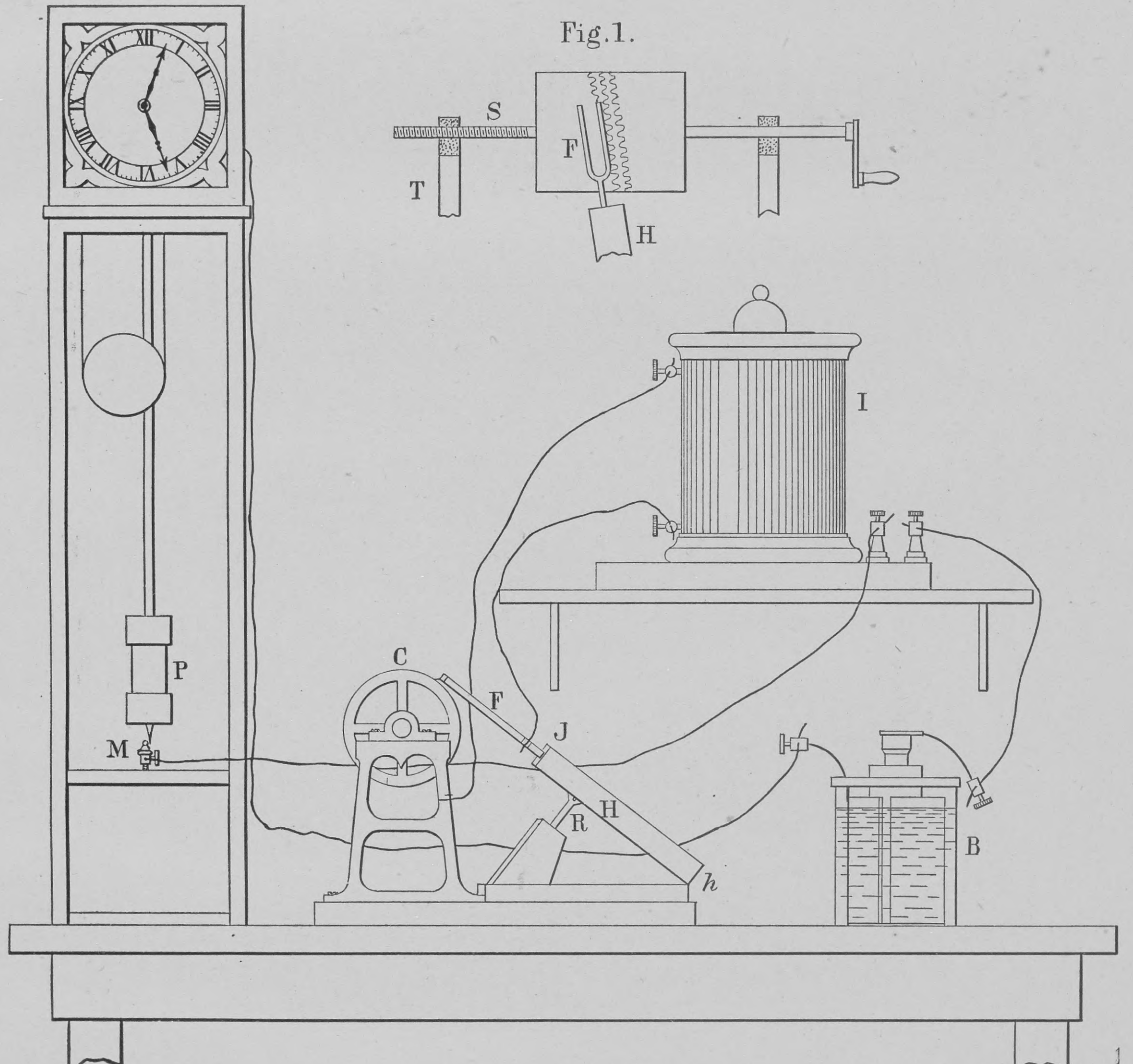


Fig.1.
S
T
F
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P
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I
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Fig. 2.

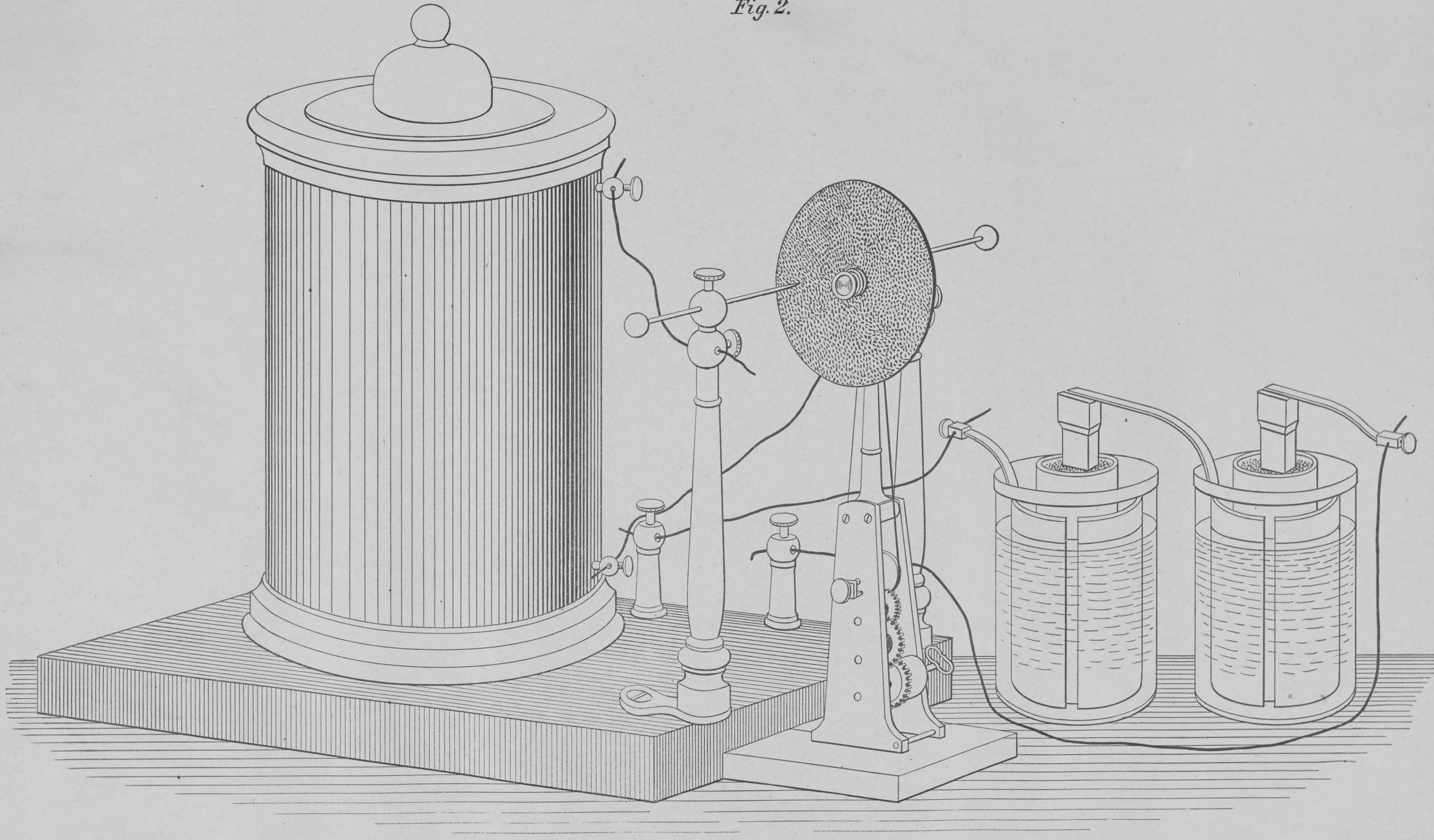

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Fig. 3.

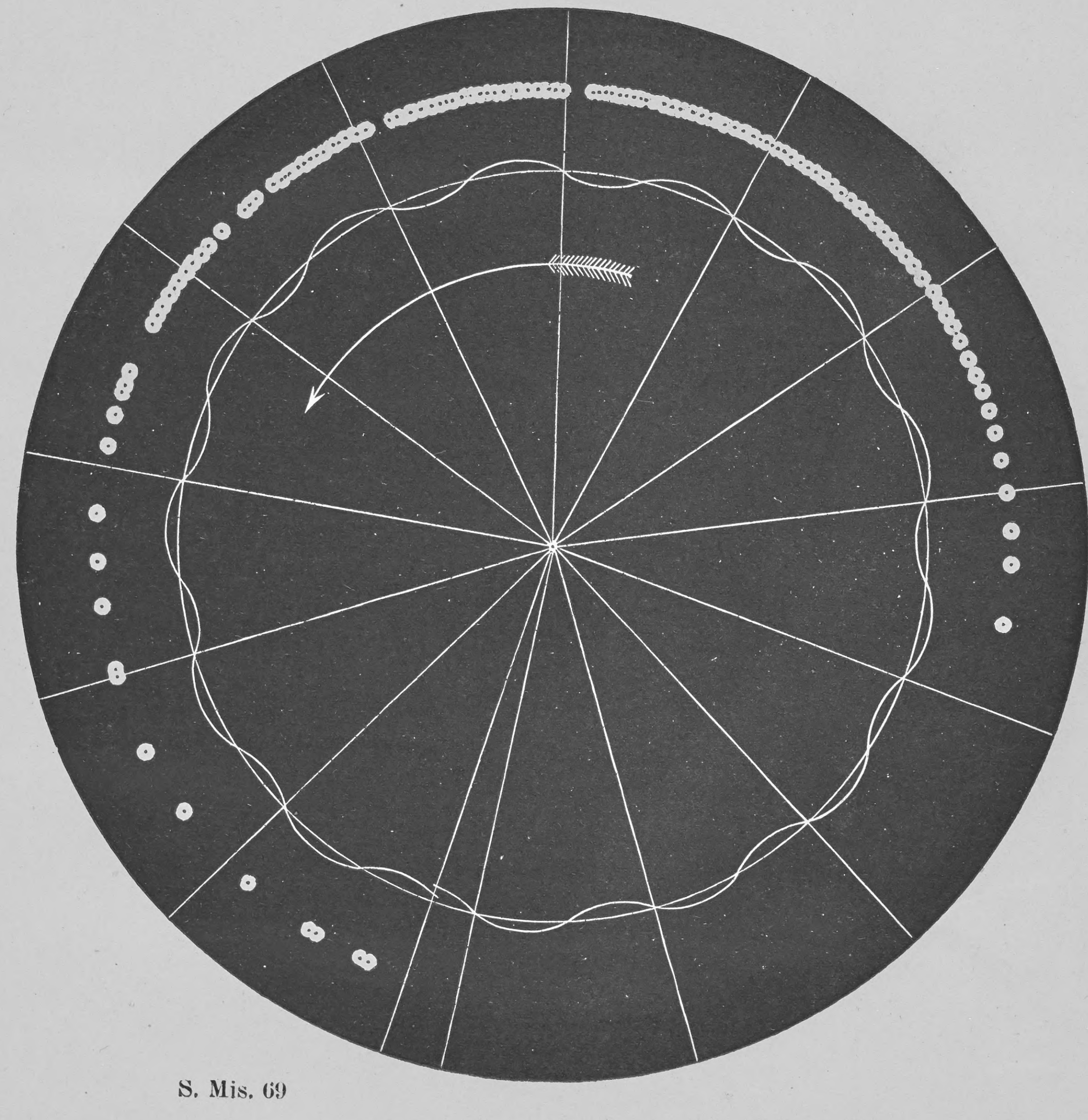

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Fig.4.

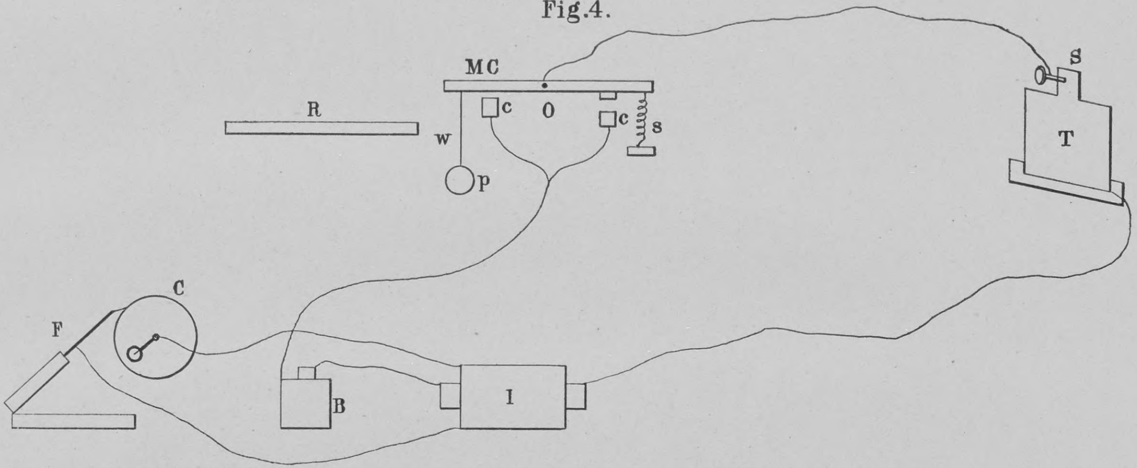
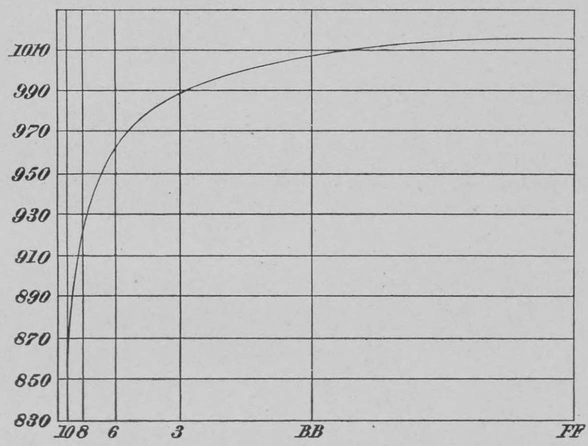


Fig. 5.



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FOURTH MEMOIR.

THE BAUMÉ HYDROMETERS.

THE BAUMÉ HYDROMETERS.

READ AT THE PHILADELPHIA MEETING, 1881.

By C. F. Chandler.

In 1768, Antoine Baumé, a chemist in Paris, published an account of two new instruments which he had devised for determining the specific gravity of liquids.

These instruments met with speedy acceptance on the part of practical men, and are now more extensively used in manufacturing establishments than any others.

Acids, alkalies, sugar solutions, petroleum oils, &c., are almost exclusively described in degrees Baumé.

The degrees on the Baumé scale are entirely arbitrary, and bear no obvious relation to the specific gravity of the liquid.

Baumé's hydrometers are instruments of even divisions. The special recommendation which has led to their extensive use among practical men is the simplicity of the numbers representing the specific gravity of the liquid. For liquids heavier than water the entire range is from zero to about 70 degrees. For liquids lighter than water, 10 to 80.

The numbers, therefore, are very easy to remember, and far more convenient on that account than the number expressing the true specific gravity, which for a liquid heavier than water would be 1 and a decimal of three figures usually, as for example 1.237.

Although Baumé described with great accuracy the method which he employed for securing the scale for his hydrometers, and it would seem, therefore, as though no difficulty existed to prevent the reproduction of his instruments, nevertheless it is a fact that among instrument-makers the scale has been so far modified from time to time that we have the greatest variety of instruments purporting to be Baumé's, each one of which has a set of degrees of an entirely different value from that exhibited by any other.

I have found twenty-three different scales, published by as many different writers, for liquids heavier than water, the highest of which gives as the value of 66° Baumé 1.8922; the lowest 1.730, no one of which can be said to be correct, or to have been obtained by following Baumé's directions.

For liquids lighter than water I have found eleven scales in which the value of 47° Baumé varies from 0.7978 to 0.7909.

Baumé's directions for the construction of his instruments are very simple, and it is almost incredible that such deviations should have occurred in connection with the instruments.

It has often been suggested that the only safe plan is to abandon the use of them entirely, and rely upon instruments which record at once the true specific gravity, referred to that of water as a unit.

The answer to this is that practical men will not abandon them, having become wedded to them, and preferring them on account of the simplicity of the numbers involved, and it would be impossible to induce them to give them up.

TABLE I.—*Value of degrees Baumé for liquids heavier than water, given by different authors.*

Degrees Baumé.	(1) Delezennes, 12.5° C. Mod.=139.97. Before 1848.	(2) Ziurek. Mod.=143.64. 1863.	(3) D'Arcet. 15° C. Mod.=143.68. 1816.	(4) Gilpin. 12.5° C. Mod.=143.83. 1797.	(5) French Codex. Holland. 12.5° C. Mod.=144. 1805.	(6) H. A. Mott, jr. 17.5° C. Mod.=144. 1877.	(7) Dalton. Mod.=144.01.	(8) Bourgougnon. 15° C. Mod.=144.32. 1878.	(9) Bineau. 15° C. Mod.=144.32. Before 1877.	(10) Vauquelin. 15° C. Mod.=144.38. 1810.	(11) Morozeau. 12.5° C. Mod.=144.38. 1830.	(12) Custom in France. Mod.=144.38. 1872.	(13) J. Kolb. 15° C. Mod.=144.38. 1872.	(14) H. Pemberton. Mod.=145. 1851.	(15) M'f'g Chem. Ass'n, U. S. A.; A. H. Elliott. 15.5° C. Mod.=145.04. 1877.	(16) Schober & Pecher. 15.5° C. Mod.=145.13. 1828.	(17) Huss, Ed'bgh Disp. Mod.=145.40. 1830.	(18) Gerlach. 17.5° C. Mod.=146.77. 1870.	(19) Chemiker Kalender, Berlin. Mod.=146.98. 1881.	(20) Baumé original scale, 15° C. Mod.=149.57. Calculated by Gerlach, 1870.	(21) Baudin. 15° C. Mod.=150.0. 1870.	(22) Francoeur. Mod.=152.05. Before 1848.	(23) Bohnenberger. 14.4° C. Mod.=156.38. Before 1865.
0	1.0000	1.000	......	1.000	1.000	1.0000	1.0000	1.0000	......	.....	1.000	1.000	1.000	1.0000	1.000	1.0000	1.0000	1.0000	1.000	1.0000	1.0000	1.000	1.000
1	1.0072	1.007	......	.....	1.007	1.0070	1.0074	1.0069	......	.....	1.008	.....	1.007	1.0069	1.005	1.0069	1.0070	1.0068	1.007	......	......	1.007	1.007
2	1.0145	1.014	......	.....	1.014	1.0141	1.0140	1.0140	......	.....	1.015	.....	1.014	1.0139	1.011	1.0139	1.0141	1.0138	1.014	......	......	1.013	1.013
3	1.0219	1.022	......	1.020	1.022	1.0213	1.0206	1.0212	......	.....	1.022	.....	1.022	1.0211	1.023	1.0211	1.0213	1.0208	1.020	......	......	1.020	1.020
4	1.0294	1.029	......	.....	1.029	1.0286	1.0268	1.0285	......	.....	1.029	.....	1.029	1.0283	1.029	1.0283	1.0286	1.0280	1.028	......	......	1.027	1.027
5	1.0370	1.036	......	.....	1.036	1.0360	1.0336	1.0358	1.036	1.023	1.036	1.023	1.037	1.0357	1.036	1.0356	1.0360	1.0353	1.034	1.0346	1.0344	1.034	1.033
6	1.0448	1.044	......	1.040	1.044	1.0435	1.0405	1.0433	......	.....	1.043	.....	1.045	1.0431	1.043	1.0431	1.0435	1.0426	1.041	......	......	1.041	1.040
7	1.0526	1.052	......	.....	1.052	1.0511	1.0477	1.0509	......	.....	1.051	.....	1.052	1.0507	1.050	1.0506	1.0511	1.0501	1.049	....	......	1.048	1.047
8	1.0606	1.060	......	.....	1.060	1.0588	1.0544	1.0586	......	.....	1.059	1.055	1.060	1.0583	1.057	1.0583	1.0588	1.0576	1.057	......	......	1.056	1.055
9	1.0687	1.067	......	1.064	1.067	1.0667	1.0614	1.0665	......	.....	1.067	.....	1.067	1.0661	1.064	1.0661	1.0666	1.0653	1.064	......	......	1.063	1.062
10	1.0769	1.075	......	.....	1.075	1.0746	1.0743	1.0744	1.075	1.076	1.075	1.076	1.075	1.0740	1.071	1.0740	1.0745	1.0731	1.072	1.0716	1.0713	1.070	1.069
11	1.0853	1.083	......	.....	1.083	1.0827	1.0809	1.0825	......	.....	1.083	.....	1.083	1.0820	1.086	1.0820	1.0825	1.0810	1.080	......	......	1.078	1.077
12	1.0937	1.091	......	1.089	1.091	1.0909	1.0887	1.0906	......	.....	1.091	1.090	1.091	1.0902	1.093	1.0901	1.0906	1.0890	1.088	......	......	1.085	1.084
13	1.1023	1.100	......	.....	1.100	1.0992	1.0953	1.0989	......	.....	1.099	.....	1.100	1.0984	1.100	1.0983	1.0988	1.0972	1.096	......	......	1.094	1.092
14	1.1111	1.106	......	....	1.108	1.1111	1.1090	1.1074	......	.....	1.107	....	1.108	1.1068	1.107	1.1067	1.1071	1.1054	1.104	......	......	1.101	1.099
15	1.1200	1.116	......	1.114	1.116	1.1163	1.1165	1.1159	1.116	1.114	1.116	1.114	1.116	1.1153	1.114	1.1152	1.1155	1.1138	1.113	1.1115	1.1110	1.109	1.107
16	1.1290	1.125	......	.....	1.125	1.1250	1.1246	1.1246	......	.....	1.125	....	1.125	1.1240	1.122	1.1239	1.1240	1.1224	1.121	......	......	1.118	1.115
17	1.1382	1.134	......	.....	1.134	1.1339	1.1330	1.1335	......	.....	1.134	.....	1.134	1.1328	1.136	1.1326	1.1326	1.1310	1.130	......	......	1.126	1.123
18	1.1475	1.143	......	1.140	1.143	1.1429	1.1410	1.1424	......	....	1.143	1.141	1.142	1.1417	1.143	1.1415	1.1414	1.1398	1.138	......	......	1.134	1.132
19	1.1570	1.152	......	...	1.152	1.1520	1.1480	1.1516	......	.....	1.152	.....	1.152	1.1507	1.150	1.1506	1.1504	1.1487	1.147	......	......	1.143	1.140
20	1.1666	1.162	......	...	1.161	1.1613	1.1549	1.1608	1.161	1.162	1.161	1.162	1.162	1.1600	1.158	1.1598	1.1596	1.1578	1.157	1.1543	1.1538	1.152	1.148
21	1.1764	1.171	......	1.170	1.171	1.1707	1.1626	1.1702	......	.....	1.170	1.170	1.171	1.1693	1.172	1.1691	1.1690	1.1670	1.166	......	......	1.160	1.157
22	1.1864	1.180	......	...	1.180	1.1803	1.1706	1.1798	......	.....	1.180	1.179	1.180	1.1788	1.179	1.1786	1.1785	1.1763	1.176	......	......	1.169	1.166
23	1.1965	1.190	......	....	1.190	1.1901	1.1876	1.1895	......	....	1.190	1.190	1.190	1.1885	1.186	1.1883	1.1882	1.1858	1.185	......	......	1.178	1.174
24	1.2068	1.199	......	1.200	1.199	1.2000	1.1956	1.1994	......	.....	1.200	1.200	1.200	1.1983	1.201	1.1981	1.1981	1.1955	1.195	......	......	1.188	1.183
25	1.2173	1.210	......	.....	1.210	1.2101	1.2108	1.2095	1.209	1.210	1.210	1.210	1.210	1.2083	1.208	1.2080	1.2082	1.2053	1.205	1.2007	1.2000	1.197	1.192
26	1.2280	1.221	......	.....	1.221	1.2203	1.2184	1.2197	......	.....	1.220	.....	1.220	1.2184	1.216	1.2182	1.2184	1.2153	1.215	......	......	1.206	1.201
27	1.2389	1.231	......	1.230	1.231	1.2308	1.2334	1.2301	......	.....	1.230	.....	1.231	1.2288	1.231	1.2285	1.2288	1.2254	1.225	......	......	1.216	1.211
28	1.2499	1.242	......	....	1.242	1.2414	1.2409	1.2407	......	.....	1.241	1.241	1.241	1.2393	1.238	1.2390	1.2394	1.2357	1.235	......	......	1.225	1.220
29	1.2612	1.252	......	.....	1.252	1.2522	1.2490	1.2514	......	.	1.252	.....	1.252	1.2500	1.254	1.2497	1.2502	1.2462	1.245	......	......	1.235	1.230
30	1.2727	1.261	......	1.261	1.261	1.2632	1.2654	1.2624	1.262	1.260	1.263	1.260	1.263	1.2608	1.262	1.2605	1.2612	1.2569	1.256	1.2509	1.2499	1.245	1.239
31	1.2844	1.275	......	.....	1.275	1.2743	1.2740	1.2735	......	.....	1.274	1.273	1.274	1.2719	1.269	1.2716	1.2724	1.2677	1.267	......	......	1.256	1.249
32	1.2962	1.286	......	...	1.286	1.2857	1.2826	1.2849	...	.....	1.285	1.284	1.285	1.2831	1.285	1.2828	1.2838	1.2788	1.278	......	......	1.267	1.260
33	1.3083	1.298	......	1.295	1.298	1.2973	1.2999	1.2964	1.296	.....	1.296	1.296	1.297	1.2946	1.293	1.2943	1.2954	1.2901	1.289	......	.....	1.277	1.270
34	1.3207	1.309	......	.....	1.309	1.3091	1.3080	1.3081	......	.....	1.308	1.307	1.308	1.3063	1.309	1.3059	1.3072	1.3015	1.300	......	......	1.288	1.281
35	1.3333	1.321	......	....	1.321	1.3211	1.3255	1.3201	1.320	1.315	1.320	1.315	1.320	1.3181	1.317	1.3177	1.3190	1.3131	1.312	1.3055	1.3042	1.299	1.291
36	1.3461	1.334	......	1.333	1.334	1.3333	1.3345	1.3323	1.332	.....	1.332	1.329	1.332	1.3302	1.334	1.3298	1.3311	1.3250	1.324	......	......	1.310	1.302
37	1.3592	1.346	......	.....	1.346	1.3458	1.3440	1.3447	1.345	.....	1.345	1.339	1.345	1.3425	1.342	1.3421	1.3434	1.3370	1.337	......	.....	1.321	1.313
38	1.3725	1.359	......	.....	1.359	1.3585	1.3612	1.3574	1.357	.....	1.358	1.359	1.357	1.3551	1.359	1.3546	1.3559	1.3494	1.349	......	......	1.333	1.325
39	1.3861	1.372	......	1.373	1.372	1.3714	1.3788	1.3703	1.370	.....	1.371	1.372	1.370	1.3679	1.368	1.3674	1.3686	1.3619	1.361	...	......	1.345	1.336
40	1.3999	1.384	......	....	1.384	1.3846	1.3884	1.3834	1.383	1.375	1.384	1.375	1.383	1.3809	1.386	1.3804	1.3815	1.3746	1.375	1.3650	1.3635	1.357	1.347
41	1.4141	1.398	......	....	1.398	1.3981	1.3977	1.3968	1.397	.....	1.397	1.399	1.397	1.3942	1.395	1.3937	1.3947	1.3876	1.388	......	......	1.369	1.359
42	1.4285	1.412	......	1.414	1.412	1.4118	1.4170	1.4104	1.410	....	1.410	1.413	1.410	1.4077	1.413	1.4072	1.4082	1.4009	1.401	......	......	1.381	1.371
43	1.4433	1.426	......	.....	1.426	1.4267	1.4265	1.4244	1.424	....	1.424	1.427	1.424	1.4215	1.422	1.4210	1.4219	1.4143	1.414	......	......	1.395	1.384
44	1.4583	1.440	......	.....	1.440	1.4400	1.4460	1.4386	1.438	....	1.438	1.441	1.438	1.4356	1.441	1.4350	1.4359	1.4281	1.428	......	......	1.407	1.396
45	1.4735	1.454	1.454	1.455	1.454	1.4545	1.4560	1.4530	1.453	1.466	1.453	1.455	1.453	1.4500	1.451	1.4493	1.4501	1.4421	1.442	1.4303	1.4204	1.420	1.404
46	1.4893	1.470	1.466	....	1.470	1.4694	1.4660	1.4678	1.468	.....	1.468	1.466	1.468	1.4646	1.470	1.4640	1.4645	1.4564	1.456	.....	......	1.434	.....
47	1.5053	1.485	1.482	.....	1.485	1.4845	1.4860	1.4829	1.483	.....	1.483	1.482	1.483	1.4795	1.480	1.4789	1.4792	1.4710	1.470	....	......	1.448	.....
48	1.5217	1.501	1.500	1.500	1.501	1.5000	1.4960	1.4983	1.498	.....	1.498	1.500	1.498	1.4949	1.500	1.4941	1.4942	1.4860	1.485	......	......	1.462	.....
49	1.5384	1.516	1.515	....	1.516	1.5158	1.5170	1.5140	1.514	....	1.514	1.515	1.514	1.5104	1.510	1.5097	1.5096	1.5012	1.500	......	....	1.476	.....
50	1.5555	1.532	1.532	.....	1.532	1.5319	1.5280	1.5301	1.530	1.524	1.530	1.532	1.530	1.5263	1.531	1.5255	1.5253	1.5167	1.515	1.5021	1.5000	1.490	.....
51	1.5730	1.549	1.550	1.547	1.549	1.5484	1.5503	1.5465	1.546	.....	1.546	1.550	1.540	1.5425	1.541	1.5417	1.5413	1.5325	1.531	....	......	1.505	.....
52	1.5909	1.566	1.566	.....	1.566	1.5652	1.5648	1.5632	1.563	.....	1.563	1.566	1.563	1.5591	1.562	1.5583	1.5576	1.5487	1.546	......	......	1.520	......
53	1.6096	1.583	1.586	.	1.583	1.5824	1.5760	1.5802	1.580	.....	1.580	1.586	1.580	1.5760	1.573	1.5752	1.5742	1.5652	1.562	......	......	1.535	......
54	1.6279	1.601	1.603	1.594	1.601	1.6000	1.5975	1.5978	1.597	....	1.598	1.603	1.597	1.5934	1.594	1.5925	1.5912	1.5820	1.578	......	......	1.551	......
55	1.6471	1.618	1.618	....	1.618	1.6179	1.6090	1.6157	1.615	1.618	1.616	1.618	1.615	1.6111	1.616	1.6101	1.6086	1.5993	1.596	1.5816	1.5789	1.567	......
56	1.6667	1.638	......	....	1.637	1.6363	1 6415	1.6340	1.634	.....	1.634	1.639	1.634	1.6292	1.627	1.6282	1.6264	1.6169	1.615	......	......	1.583	......
57	1.6868	1.659	......	1.659	1.656	1.6551	1.6520	1.6527	1.652	....	1.653	1.660	1.652	1.6477	1.650	1.6467	1.6446	1.6349	1.634	....	......	1.600	......
58	1.7074	1.678	......	...	1.676	1.6744	1.6750	1.6719	1.671	.....	1.672	1.682	1.671	1.6666	1.661	1.6656	1.6632	1.6533	1.653	......	......	1.617	......
59	1.7285	1.698	......	....	1.695	1.6941	1.6993	1.6915	1.691	....	1.691	1.703	1.691	1.6860	1.683	1.6849	1.6823	1.6721	1.671	......	......	1.634	......
60	1.7501	1.718	1.717	1.717	1.715	1.7142	1.7120	1.7115	1.711	1.725	1.711	1.725	1.711	1.7058	1.705	1.7047	1.7019	1.6914	1.690	1.6698	1.6666	1.652	......
61	1.7722	1.739	......	.....	1.736	1.7349	1.7360	1.7321	1.732	.....	1.732	1.744	1.732	1.7261	1.727	1.7250	1.7220	1.7111	1.709	......	......	1.670	......
62	1.7950	1.760	......	.	1.758	1.7560	1.7570	1.7531	1.753	....	1.753	1.764	1.753	1.7469	1.747	1.7457	1.7427	1.7313	1.729	......	......	1.689	......
63	1.8184	1.782	......	1.779	1.779	1.7777	1.7774	1.7748	1.774	....	1.775	1.783	1.774	1.7682	1.767	1.7669	1.7640	1.7520	1.750	......	......	1.708	......
64	1.8423	1.804	......	.....	1.801	1.7999	1.8048	1.7968	1.796	....	1.797	1.803	1.796	1.7901	1.793	1.7888	1.7858	1 7731	1.771	......	......	1.727	......
65	1.8669	1.827	......	...	1.823	1.8227	1.8233	1.8194	1.819	.. .	1.819	1.822	1.819	1.8125	1.814	1.8111	1.8082	1.7948	1.793	1.7685	1.7646	1.747	......
66	1.8922	1.850	1.849*	1.848	1.847	1.8461	1.8460	1.8427	1.8426	1.842	1.842	1.842	1.842	1.8354	1 835	1.8340	1.8312	1.8171	1.815	1.7897	1.786*	1.767	*1.730
67	1.9180	1.874	......	.....	1.872	1.8701	......	1.8665	......	.....	1.866	....	.....	1.8589	......	1.8574	1.8548	1.8398	1.839	......	......	1.788	......
68	1.9447	1.898	......	....	1.897	1.8947	....	1.8909	......	.....	1.891	....	.....	1.8831	......	1.8815	1.8790	1.8632	1.864	......	......	1.809	......
69	1.9721	1.922	......	1.920	1.921	1.9200	......	1.9161	......	.....	1.916	....	.....	1.9079	......	1.9062	1.9038	1.8871	1.885	......	......	1.831	......
70	2.0003	1.947	.....	.....	1.946	1.9459	......	1.9418	......	.....	1.942	....	.....	1.9333	.....	1.9316	1.9291	1.9117	1.909	1.8796	1.8750	1.854	......
71	......	1.973	......	...	1.974	1.9726	......	1.9683	......	.....	1.968	....	.....	1.9595	.....	1.9577	1.9548	1.9370	1.935	.....	.....	1.877	......
72	.....	2.000	......	2.000	2.002	2.0000	......	1.9955	......	.....	1.995	....	.....	1.9863	......	1.9844	1.9809	1.9629	1.960	......	......	1.900	......
73	......	.....	......	.....	2.031	......	......	2.0235	......	.....	2.023	....	.....	2.0139	......	2.0119	2.0073	1.9895	....	......	......	1.924	......
74	......	.....	......	.....	2.059	.....	......	2.0523	......	.....	2.052	....	.....	2.0422	......	2.0402	2.0340	2.0167	....	......	......	1.949	......
75	......	.....	......	.....	2.087	.....	......	2.0819	......	.....	2.081	....	.....	2.0714	......	2.0693	2.0610	2.0449	....	......	......	1.974	......
76	......	.....	......	.....	.....	.....	......	......	......	.....	.....	....	....	......	....	2.0992	......	......	...	......	......	2.000	......

*Calculated by the compiler.

NOTE.—Where the modulus was not given, it was calculated by the formula: $n=\frac{P\times d}{P-1}$ in which n = modulus; P = specific gravity = Baumé degree. 66 was taken for "d" whenever the corresponding Sp. Gr. appeared.

The only thing to be done is to correct the inaccuracies and establish by some competent authority an authorized and accepted standard of values for the Baumé scales.

Baumé's methods were first described in *L'Avant Coureur* towards the close of 1768. They have been repeated in the several editions of his Éléments de Pharmacie. In the eighth edition of this work, published in Paris in 1797, he states that he constructed his instruments in this way:

(1) *For the hydrometer for liquids heavier than water* he prepared a solution of salt containing fifteen (15) parts of salt by weight in eighty-five (85) parts of water by weight. He describes the salt as "very pure" and "very dry," and states that the experiments should be made in a cellar in which the temperature is 10° Reaumur, equivalent to 12.5° Centigrade and to 54.5° Fahrenheit. The zero on the scale indicates the point to which the instrument sinks in distilled water (at the temperature above stated), the 15 mark the point to which it sinks in the 15 per cent. salt solution. With a pair of dividers the space between "0" and "15" is divided into fifteen equal parts, and degrees of the same size are continued above "15."

Baumé's idea was that each additional degree on this scale would indicate one additional per cent. of salt, which of course is not quite correct, but the directions given are sufficiently simple to enable any person to reproduce the instrument.

(2) *For the hydrometer for liquids lighter than water* he uses a 10 per cent. solution of salt prepared in the same way, and by means of it fixes the zero point on the hydrometer. He uses distilled water for the "10" point, and obtains a scale as in the case of the other instrument, but running in the opposite direction.

With so simple and direct a statement as this it is remarkable that it has been possible to get so far away from the true Baumé scales. In looking over the literature of the subject I find that these discrepancies have arisen from various causes—either neglect to follow Baumé's directions, or a deliberate attempt to improve the scales.

(*a*) Baumé conveys the idea that each degree represents 1 per cent. of salt, and he even suggests that in order to obviate errors due to irregularities in the stem of the instrument, a series of solutions may be prepared, the first containing 1 per cent. of salt and 99 per cent. of water, the second 2 per cent. of salt and 98 per cent. of water, and so on, and that the degrees 1, 2, &c., can be marked by the use of these solutions.

(*b*) Acting still further on this suggestion of Baumé, many instrument makers gave up preparing the 15 per cent. salt solution altogether for fixing the "15" mark, using instead the 10 per cent. solution and fixing by it the "10" mark, thus making one solution answer for both instruments.

(*c*) It was found at an early day that oil of vitriol generally stood at about 66 on the Baumé instrument; so many instrument makers fixed the 66 mark by immersing the instrument in oil of vitriol. As a matter of fact oil of vitriol is a variable substance. It never contains 100 per cent. of sulphuric acid—usually only from 92 to 96 per cent. It consequently has a variable specific gravity, and its use for the 66° mark introduces varying errors.

SCALES FOR LIQUIDS HEAVIER THAN WATER.

I submit herewith a table—"Table No. I"—containing twenty-three different scales of values for the degrees on the instrument for liquids heavier than water, and another table—"Table No. II"—containing the eleven scales for liquids lighter than water.

METHODS EMPLOYED IN SECURING THE SCALES GIVEN IN TABLE I.

(1) Delezennes. 66°=1.8922. The mark for 10° was found by a 10 per cent. salt solution at 10° R. (Wagner Jahresb., 1869, vol. 15, 236.) This scale appears in Journal de Phys., vol. 94, 204; Bache & McCulloh, 1848, 116; Dingler's Polyt. Journal, 1865, 2 vol., 176, 455; Handwörterbuch der Chemie, 1859, vol. 2, 1, 179; Knapp, Chem. Tech.

(2) Ziurek. 66°=1.850. No method given. This scale appears in Technologische Tabellen, 1863, 35.

(3) D'Arcet. 66°=1.849 (calculated). The point 66° B. was obtained in sulphuric acid of specific gravity 1.830, but it is assumed that it is not pure hydrate, but contains about 6 to 7 per cent.

more water than the hydrate $H_2 SO_4$. (Muspratt, vol. 6, 357.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872; Muspratt, 1879, vol. 6, 359.

(4) Gilpin. 66°=1.848. The mark 10° was found by a 10 per cent. salt solution at 10° R. (Wagner, Jahresb., 1869, vol. 15, 236.) This scale appears in Henry, 1810; Children, 1819; Ann. de Chimie, vol. 23, 1797; Handwörterbuch, vol. 2, 1; Bache & McCulloh, 1848; Knapp, Chem. Tech., vol. 1, part 5; Journal de Physique, 1797.

(5) French Codex (Holland). 66°=1.847. In the Holland scale, the 10° was obtained by a 10 per cent. common salt solution at 10° R. (Bache & McCulloh. Reports on Sugar and Hydrometers, 1848, 84.) This scale appears in U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, and 14th editions; Pharmacopœa Batava, 1805; Bache & McCulloh, 1848; Neues Handwörterbuch, 1871; Dingler's Polyt. Journal, 1870.

(6) H. A. Mott, jr. 66°=1.8461. Was deduced by Doctor Pyle, of Philadelphia, and the table calculated to 0.5 by Doctor Mott. (Letter from Dr. M. to Dr. C. F. C., Nov. 8, 1881.) This scale appears in Mott, Chemist's Manual, 1877.

(7) Dalton. 66°=1.8460. The point 66° was obtained in sulphuric acid of specific gravity 1.830 (see D'Arcet). (Muspratt, 1879, vol. 6, 357.) This scale appears in Muspratt's Technische Chemie, 1879, vol. 6.

(8) Bourgougnon. 66°=1.8427. This table is calculated according to the formula—

$$P=\frac{144.3}{144.3-d}$$

in which P=density; d=degree Baumé. This formula is obtained when Gay-Lussac's method is used with sulphuric acid of specific gravity 1.8427 at 15° C. (Tucker, Manual of Sugar Analysis, 1881, pp. 108, 109.) This scale appears in Proc. Am. Chem. Soc., vol. 1, No. 5, 1878; Tucker, Manual of Sugar Analysis, 1881.

(9) Bineau. 66°=1.8426. In Bineau's tables, which Otto has calculated for 15° C. according to Bineau's own statements, the specific gravity of the sulphuric acid (Schwefelsaürehydrates) at 15° C. =1.8426. (Wagner, Jahresb., 1869, vol. 15, 238.) This scale appears in Muspratt, 1879, vol. 6, 358; Agendas Dunod, 1877; Lunge, 1879, vol. 1.

(10) Vauquelin. 66°=1.842. The point 66° was obtained in sulphuric acid of specific gravity 1.830 (see D'Arcet). (Muspratt, 1879, vol. 6, 357.) This scale appears in Ann. de Chimie et Physique, 1 series, vol. 76; Bull. Ind. de Mülhouse, 1872; Muspratt, 1879, vol. 6, 359.

(11) Morozeau. 66°=1.842. Calculated by Morozeau by the formula

$$y=\frac{dd'(n'-n)}{n'd'-nd-x(d'-d)}$$

n, n', and x are the degrees of the instrument corresponding to the specific gravities, d, d', and y. The 66°=1.842 at 10° R. This number is accepted because it corresponds to the highest specific gravity of "acide sulfurique hydreux," because it is given by Thénard and because it seems generally accepted. In giving to x the values 1, 2, 3, up to 75, the corresponding values of y have therefrom been deduced. (Journal de Pharmacie, Paris, 1830, vol. 16, p. 488.) This scale appears in Journal de Pharmacie, vol. 16, 488; Knapp, Chem. Technologie; École Centrale Lyonnaise.

(12) Custom in France. 66°=1.842. This table is based on Vauquelin's table. (Bull. Soc. Ind. de Mülhouse (42) 1872, p. 211.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872.

(13) J. Kolb. 66°=1.842. 66°=pure sulphuric acid of specific gravity 1.842. (Lunge Soda Industrie, 1879, vol. 1, 24.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872; Roscoe and Schorlemmer, 1877; Wurtz, Dict. de Chimie, 1876; Lunge, Soda Industrie, 1879, vol. 1; Deut. Chem. Kalendar, Dresden, 1877; Wagner, Chem. Tech., 1875; Muspratt, 1879, vol. 6, 359. Nos. 10, 11, 12, and 13 all give 66° Baumé=1.842, though differing in other terms.

(14) H. Pemberton. 66°=1.8354. Calculated by H. Pemberton in 1851, and adopted as standard by the Philadelphia College of Pharmacy the same year. This scale appears in U. S. Dispensatory, 12th, 13th, and 14th editions.

(15) Manufacturing Chemists' Association, U. S. A. 66°=1.835. Calculated by A. H. Elliott from the data given by the committee on "What is oil of vitriol?" in 1875,

66° B= H_2SO_4	93.5
H_2O	6.5
	100.0

This scale appears in a separate sheet published by the association. In a report of the Commission on "What is oil of vitriol?" previously published, a table differing slightly from this is published.

(16) Schober and Pecher. 66°=1.8340. The mark 10° was obtained by a 10 per cent. salt solution of specific gravity 1.074 and the scale calculated by the formula

$$S=\frac{10\,p}{10\,p+n\,(p-1)}$$

in which S=specific gravity of the fluid, p=specific gravity of salt solution, n=degrees. (Dingler's Polyt. J., 1828, vol. 27, 63.) This scale appears in Dingler's Polyt. J., vol. 27, 63; Hoffmann-Schaedler Tabellen, 1877; Knapp, Chem. Tech.; E. L. Schubarth, vol. 1, 47.

(17) Huss, Edinburgh Dispensatory. 66°=1.8312. Calculated by Huss and published in Duncan's Ed'b'gh Disp., 1830. This scale appears in Duncan's Ed'b'gh Disp., 1830; U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, and 14th editions.

(18) Gerlach. 66°=1.8171. Based on a 10 per cent. salt solution of specific gravity 1.07311 at 14° R. (Dingler Polyt. J., 1870, 198, 315.) This scale appears in Dingler's Polyt. J., 1870; Post, Chem. Tech. Analyse, 1881, Part 1, 438; Lunge, Soda Industrie, 1879, vol. 1.

(19) Chemiker Kalender. Berlin. 66°=1.815. No method stated. This scale appears in Chem. Kalender, Berlin, Dr. Biedermann, 1881.

(20) "Baumé Original Scale." As calculated by Gerlach, 1870. 66°=1.7897. Based on the specific gravity of a 15 per cent. salt solution *in vacuo* at 15° C.=1.11146. This scale appears in Dingler's Polyt. J., 1870,vol. 198, 316.

(21) Baudin. 66°=1.786 (calculated). A 15 per cent. salt solution of specific gravity 1.111 was employed for the 15 mark, at 15° C. (Chemical News, 1870, vol. 21, 54.) This scale appears in Chemical News, 1870, vol. 21, 54.

(22) Francœur. 66°=1.767. The 15 mark was obtained by a 15 per cent. solution of rock salt dissolved in distilled water at maximum density specific gravity=1.1094. (Francœur, Mémoire, sur l'Aréomètrie, 1842, Paris, 26.) This scale appears in Watts' Dict., vol. 3, 209; Johnson's Cycl., vol. 2, 1062; Fownes' Chemistry, 12th ed.; Ure's Dict., vol. 1; Handwörterbuch der Chemie, vol. 2, 1; Knapp, Chem. Tech.; Bache & McCulloh, 1848.

(23) Bohnenberger. 66°=1.730 (calculated). Probably a 15 per cent. salt solution at 11.5° R. was employed for the 15 mark. (Wagner, Jahresb., 1869, vol. 15, 235.) This scale appears in Handwörterbuch der Chemie, vol. 2, 1; Practical Magazine; Dingler's Polyt. J., 1865, vol. 176; Tüb. Blätter, vol. 2, 457; Knapp, Chem. Technology.

THE TRUE SCALE FOR LIQUIDS HEAVIER THAN WATER.

As no one of these twenty-three scales had been obtained by following Baumé exactly, it was deemed advisable to repeat his experiments.

Three solutions were prepared by following exactly the directions of Baumé, each one containing 15 per cent. of salt and 85 per cent. of water by weight. For the first solution chemically pure sodium chloride was employed; for the second, "solar salt," from Syracuse; for the third, "factory-filled dairy salt," from Syracuse. The specific gravity of these solutions was carefully determined at 10° Reaumur. The results are given in Table III, together with the results obtained by several friends who have repeated this experiment, and also of several chemists who have published their results.

TABLE II.—*Value of degrees Baumé for liquids lighter than water, given by different authors.*

[Compiled by C. F. Chandler and F. G. Wiechmann, 1882.]

Degrees Baumé.	Francœur. 12.5° C. Mod.=145.98. 1842. (?)[1]	Gilpin. 12.5° C. Mod.=145.26. 1794.[2]	Chemiker Kalendar. 15.62° C. Mod.=145.26. 1881.[3]	Schober & Pecher. 15.62° C. Mod.=145.17. 1828.[4]	Holland. 12.5° C. Mod.=144.37. 1805.[5]	French Codex. 12.5° C. Mod.=143.48. 1850. (?)[6]	Brix. Mod.=143.13, before 1865.[7]	Delezennes. 12.5° C. Mod.=140.11, before 1848.[8]	Huss, Ed'b'gh Disp. Mod.=140.11. 1830.[9]	Ziurek. 12.5° C. Mod.=140.03. 1863.[10]	Pemberton. Mod.=139.94. 1851.[11]
10	1.0000	1.000	1.000	1.0000	1.000	1.000	1.0000	1.0000	1.0000	1.000	1.0000
11	0.9932	0.990	0.993	0.9931	0.993	0.993	0.9928	0.9929	0.9930	0.993	0.9929
12	0.9865	0.985	0.986	0.9864	0.987	0.986	0.9866	0.9859	0.9861	0.986	0.9859
13	0.9799	0.977	0.979	0.9797	0.980	0.979	0.9800	0.9790	0.9792	0.979	0.9790
14	0.9733	0.970	0.973	0.9731	0.974	0.973	0.9729	0.9722	0.9724	0.972	0.9722
15	0.9669	0.963	0.967	0.9666	0.967	0.966	0.9666	0.9655	0.9657	0.966	0.9655
16	0.9605	0.955	0.960	0.9603	0.961	0.960	0.9605	0.9589	0.9591	0.959	0.9589
17	0.9542	0.949	0.954	0.9539	0.954	0.953	0.9535	0.9524	0.9526	0.952	0.9523
18	0.9480	0.942	0.948	0.9477	0.948	0.947	0.9470	0.9460	0.9462	0.946	0.9459
19	0.9420	0.935	0.942	0.9416	0.941	0.941	0.9417	0.9396	0.9399	0.940	0.9395
20	0.9359	0.928	0.935	0.9355	0.935	0.935	0.9343	0.9333	0.9336	0.933	0.9333
21	0.9300	0.922	0.929	0.9295	0.929	0.929	0.9283	0.9272	0.9274	0.927	0.9271
22	0.9241	0.915	0.924	0.9236	0.923	0.923	0.9221	0.9211	0.9212	0.921	0.9210
23	0.9183	0.909	0.918	0.9177	0.917	0.917	0.9178	0.9151	0.9151	0.915	0.9150
24	0.9125	0.903	0.912	0.9120	0.911	0.911	0.9112	0.9091	0.9091	0.909	0.9090
25	0.9068	0.897	0.906	0.9063	0.906	0.905	0.9067	0.9033	0.9032	0.903	0.9032
26	0.9012	0.892	0.901	0.9007	0.900	0.900	0.8997	0.8975	0.8974	0.898	0.8974
27	0.8957	0.886	0.895	0.8951	0.895	0.894	0.8949	0.8918	0.8917	0.892	0.8917
28	0.8902	0.880	0.889	0.8896	0.889	0.889	0.8900	0.8861	0.8860	0.886	0.8860
29	0.8848	0.874	0.884	0.8842	0.884	0.883	0.8824	0.8806	0.8804	0.881	0.8805
30	0.8795	0.867	0.879	0.8788	0.878	0.878	0.8773	0.8751	0.8748	0.875	0.8750
31	0.8742	0.861	0.873	0.8735	0.873	0.872	0.8720	0.8696	0.8693	0.870	0.8695
32	0.8690	0.856	0.868	0.8683	0.868	0.867	0.8664	0.8643	0.8638	0.864	0.8641
33	0.8639	0.852	0.863	0.8632	0.863	0.862	0.8611	0.8590	0.8584	0.859	0.8588
34	0.8588	0.847	0.858	0.8580	0.858	0.857	0.8583	0.8537	0.8531	0.854	0.8536
35	0.8538	0.842	0.853	0.8530	0.852	0.852	0.8526	0.8486	0.8479	0.849	0.8484
36	0.8488	0.837	0.848	0.8480	0.847	0.847	0.8466	0.8435	0.8428	0.844	0.8433
37	0.8439	0.832	0.843	0.8431	0.842	0.842	0.8436	0.8384	0.8378	0.838	0.8383
38	0.8391	0.827	0.838	0.8382	0.837	0.837	0.8373	0.8334	0.8329	0.833	0.8333
39	0.8343	0.822	0.833	0.8334	0.832	0.832	0.8306	0.8285	0.8281	0.829	0.8284
40	0.8295	0.817	0.829	0.8287	0.828	0.827	0.8272	0.8236	0.8233	0.824	0.8235
41	0.8249	0.814	0.824	0.8239	0.823	0.823	0.8237	0.8188	0.8186	0.819	0.8187
42	0.8202	0.811	0.819	0.8193	0.819	0.818	0.8164	0.8141	0.8139	0.814	0.8139
43	0.8156	0.808	0.815	0.8147	0.814	0.813	0.8125	0.8094	0.8093	0.809	0.8092

Degrees Baumé.	Francœur. 12.5° C. Mod.=145.98. 1842. (?)[1]	Gilpin. 12.5° C. Mod.=145.26. 1794.[2]	Chemiker Kalendar. 15.62° C. Mod.=145.26. 1881.[3]	Schober & Pecher. 15.62° C. Mod.=145.17. 1828.[4]	Holland. 12.5° C. Mod.=144.37. 1805.[5]	French Codex. 12.5° C. Mod.=143.48. 1850. (?)[6]	Brix. Mod.=143.13, before 1865.[7]	Delezennes. 12.5° C. Mod.=140.11, before 1848.[8]	Huss, Ed'b'gh Disp. Mod.=140.11. 1830.[9]	Ziurek. 12.5° C. Mod.=140.03. 1863.[10]	Pemberton. Mod.=139.94. 1851.[11]
44	0.8111	0.805	0.810	0.8102	0.810	0.809	0.8084	0.8017	0.8047	0.802	0.8045
45	0.8066	0.802	0.806	0.8057	0.805	0.804	0.8041	0.8001	0.8001	0.800	0.8000
46	0.8022	0.799	0.801	0.8013	0.800	0.800	0.7995	0.7956	0.7956	0.796	0.7954
47	0.7978	0.797	0.797	0.7969	0.796	0.795	0.7946	0.7911	0.7911	0.791	0.7909
48	0.7935	0.795	0.792	0.7925	0.792	0.791	......	0.7866	0.7866	0.787	0.7865
49	0.7892	0.793	0.788	0.7882	0.787	0.787	......	0.7823	0.7821	0.782	0.7821
50	0.7849	0.791	0.784	0.7839	0.782	0.783	......	0.7779	0.7777	0.778	0.7777
51	0.7807	....	0.781	0.7797	.....	0.778	......	......	0.7733	0.773	0.7734
52	0.7766	.....	0.776	0.7756	....	0.774	......	......	0.7689	0.769	0.7692
53	0.7725	....	0.771	0.7714	....	0.770	.....	......	0.7646	0.765	0.7650
54	0.7684	...	0.769	0.7674	.....	0.766	......	......	0.7603	0.760	0.7608
55	0.7644	.....	0.763	0.7633	....	0.762	......	......	0.7560	0.756	0.7567
56	0.7604	.....	0.759	0.7593	....	0.758	......	......	0.7518	0.752	0.7526
57	0.7565	.....	0.755	0.7554	....	0.754	......	......	0.7476	0.748	0.7486
58	0.7526	....	0.751	0.7515	....	0.750	......	......	0.7435	0.744	0.7446
59	0.7487	.....	0.748	0.7476	.....	0.746	......	......	0.7394	0.739	0.7407
60	0.7449	.....	0.744	0.7438	....	0.742	......	......	0.7354	0.735	0.7368
61	0.7411	....	0.740	0.7399	....	0.738	......	......	0.7314	.....	0.7329
62	......	.....	0.736	0.7362	....	0.735	......	......	0.7275	....	0.7290
63	......	......	......	......	.....	0.731	......	......	.....	......	0.7253
64	......	......	......	......	.....	0.727	......	......	......	.....	0.7216
65	......	......	......	......	.....	0.724	......	......	......	.. .	0.7179
66	......	......	......	......	.....	0.720	......	......	......	....	0.7142
67	......	......	......	......	.....	0.716	......	......	......	.....	0.7106
68	......	......	......	......	....	0.713	......	......	......	.....	0.7070
69	......	......	......	......	.....	0.709	......	......	......	....	0.7035
70	......	...	......	......	...	0.706	......	......	......	.....	0.7000
71	.....	.....	......	......	.....	0.702	......	.....	......	.....	0.6965
72	......	......	......	......	.....	0.699	......	......	......	.....	0.6930
73	.....	......	......	......	.....	0.696	......	......	......	....	0.6896
74	......	......	......	......	.....	0.692	......	......	......	.....	0.6863
75	......	.....	......	......	.....	0.689	......	......	......	.....	0.6829
76	......	......	......	......	....	0.686	......	......	......	....	0.6796
77	......	......	......	......	.....	0.682	......	......	......	.....	0.6763

[1] Bache & McCulloh, 1848; Watt's Dict., 1865, vol. III; U. S. Petroleum Ass'n, 1864; Handwörterbuch der Chemie, 1859, vol. II, 1; Dingler's Poly. Journal, 1870, vol. 198; Tucker, Manual of Sugar Analysis, 1881; Johnson's Cycl., Vol. II, 1876; Fowne's Chemistry, 12th ed., 1877; Ure's Dict., vol. I, 1867; Neues Handwörterbuch, 1871, Vol. I; Deut. Chem. Kalender, Dresden, 1877. [2] Trans. Philos., 1794; Annales de Chimie, 1797; Children, 1819; Bache & McCulloh, 1848. [3] Chemiker Kalender, Berlin, 1881, 1882. [4] Hoffmann-Schaedler, Tabellen, 1877; Dingler's Poly. Journal, Vol. XXVII, 1828. [5] Bache & McCulloh, 1848; Pharmacopœa Batava, 1805 [6] U. S. Dispensatory, 11th, 12th, 13th, 14th eds.; Neues Handwörterbuch, 1871, Vol. I. [7] Bolley Handbuch der Chem. Technologie, 1865. [8] Bache & McCulloh, 1848; Handwörterbuch der Chemie, Vol. II, 1, 1859. [9] Duncan's Edinburgh Disp., 1830; U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, 14th eds. [10] Ziurek, Technologische, Tabellen & Notizen, 1863. [11] Philadelphia Coll. Pharmacy; U. S. Disp., 12th, 13th, 14th eds.; Mott, Chemist's Manual, 1877.

NOTE.—The modulus for each scale was calculated by the formula, $n=\frac{P\,(d-10)}{1-P}$, in which n = modulus; d = Baumé degree; P = specific gravity.

The calculations were in each case made on the 47th Baumé degree.

TABLE III.—*Specific gravity of a 15 per cent. solution of common salt at* 10° *R.* (=12.5° *C.*=54.5° *F.*).

No.	Specific gravity.	By whom calculated.
1	1.1122	Chandler and Wiechmann.
2	1.1121	do.
3	1.1120	do.
4	1.1122	do.
5	1.1126	do.
6	1.1121	Prof. Henry Morton.
7	1.1119	Dr. Hermann Endemann.
8	1.1110	Dr. Arno Behr.
9	1.1110	M. Baudin (Chem. News, 1870, XXI, 54).
10	1.110725	Prof. Coulier, *Ibid.*
11	1.11146	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 1).
12	1.1160	E. Soubeiran (Traité de Pharmacie 3ième ed., 1847, I, 13).
13	1.1094	Francoeur (Mémoire sur l'Aréomètrie, Paris, 1842, 26).
	1.1118988	Average.

NOTE.—1 and 2 were chemically pure salt; 3 and 4 were Syracuse solar salt; 5 was Syracuse factory-filled dairy salt.

It should be remarked that in the above table the number by Baudin was obtained by weighing the solution at 15° centigrade instead of 12.5, and Gerlach's result was obtained by weighing at 14° centigrade, and *calculating* what the specific gravity would be at 15° centigrade *in vacuo*.

Francœur determined his specific gravity at the maximum density of water.

None of these determinations were rejected, however, in making up the table, as the numbers are so nearly alike. We may fairly assume that the average is practically 1.1119.

Table IV exhibits a scale which has been carefully calculated by Mr. Wiechmann from the actual average as given on Table III, by the formulæ

$$n=\frac{P\times d}{P-1} \qquad P=\frac{n}{n-d}$$

In which P=the specific gravity; d=the Baumé degree; n=the modulus.

TABLE IV.—*Value of degrees Baumé calculated from* 0°=1, *and* 15°=1.1118988 *by the modulus* 149.04969, *the experimental work having been conducted in exact accordance with Baumé's original directions.*

[Temperature 10° R. =12.5° C. =54.5° F.]

Baumé degrees.	Specific gravity.	Baumé degrees.	Specific gravity.	Baumé degrees.	Specific gravity.	Baumé degrees.	Specific gravity.
0	1.00000	20	1.15497	39	1.35438	58	1.63701
1	1.00675	21	1.16399	40	1.36680	59	1.65519
2	1.01360	22	1.17316	41	1.37945	60	1.67378
3	1.02054	23	1.18246	42	1.39234	61	1.69279
4	1.02757	24	1.19192	43	1.40547	62	1.71223
5	1.03471	25	1.20153	44	1.41885	63	1.73213
6	1.04194	26	1.21129	45	1.43248	64	1.75250
7	1.04927	27	1.22122	46	1.44638	65	1.77335
8	1.05671	28	1.23131	47	1.46056	66	1.79470
9	1.06426	29	1.24156	48	1.47501	67	1.81657
10	1.07191	30	1.25199	49	1.48975	68	1.83899
11	1.07968	31	1.26260	50	1.50479	69	1.86196
12	1.08755	32	1.27338	51	1.52014	70	1.88551
13	1.09555	33	1.28436	52	1.53580	71	1.90967
14	1.10366	34	1.29552	53	1.55179	72	1.93446
15	1.11189	35	1.30688	54	1.56812	73	1.95989
16	1.12025	36	1.31844	55	1.58479	74	1.98601
17	1.12873	37	1.33021	56	1.60182	75	2.01283
18	1.13735	38	1.34218	57	1.61923	76	2.04038
19	1.14609						

It will be seen by comparing Table IV with Table I that this scale corresponds most closely with No. 20, which is entitled "Baumé's original scale," and which was calculated by Gerlach in 1870—Dingler's Pol. J., vol. 198, 314—and was based upon the specific gravity 1.11146 for the 15 per cent. salt solution. The observation, however, was made at 14° centigrade and was then calculated for 15° centigrade in vacuo, while Baumé's directions are to use a 15 per cent. salt solution at 10° Réaumur in the atmosphere. It will be seen that neither this nor any other of the twenty-three scales published in Table I has been obtained by strictly following Baumé's directions.

SCALES FOR LIQUIDS LIGHTER THAN WATER.

For the purpose of ascertaining the exact value of Baumé's degrees for liquids lighter than water, three 10 per cent. salt solutions were carefully prepared, using as before chemically pure salt, "Solar" Syracuse salt, and Syracuse "Factory-filled dairy salt." The results are exhibited in Table V, together with results obtained by other chemists.

TABLE V.

Specific gravity of a 10 per cent. solution of common salt at 10° R. (=12.5° C. =54.5° F.)

1.	1.0738	Chandler and Wiechmann.
2.	1.0737	Chandler and Wiechmann.
3.	1.0741	Chandler and Wiechmann.
4.	1.07303	Schober and Pecher (Dingl. Pol. J., 1828, XXVII, 65).
5.	1.07518	Schober and Pecher.
6.	1.07372	Schober and Pecher.
7.	1.073464	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 8).
8.	1.073405	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 8).
9.	1.07350	Francœur (Mémoire sur l'Aréomètrie, Paris, 1842, 26).
	1.0737665	Average.

NOTE.—1 was chemically pure salt; 2 was Syracuse solar salt; 3 was Syracuse factory-filled dairy salt; 4 was rock salt; 5 was chemically pure salt; 6 was commercial salt.

The average of these determinations gives as the specific gravity of a 10 per cent. salt solution 1.0737665, and the modulus is=145.56289, computed according to the formula

$$n=\frac{P\,(d-10)}{1-p}$$

in which P=the specific gravity, d=the Baumé degree, n=the modulus.

With the use of this modulus the following table (Table VI) has been calculated by the formula

$$P=\frac{n}{(n-10)+d}\cdot$$

in which P=the specific gravity, d=the Baumé degree, n=the modulus.

TABLE VI.—*Value of degrees Baumé calculated from* 0°=1.0737665 *and* 10°=1 *by the modulus* 145.56289, *the experimental work having been conducted in exact accordance with Baumé's original directions.*

[Temperature 10° R.=12.5° C=54.5° F.]

Baumé degree.	Specific gravity.
10	1.00000
15	0.96679
20	0.93571
25	0.90657
30	0.87919
35	0.85342
40	0.82912
45	0.80616
50	0.78443
55	0.76385
60	0.744432
65	0.72577
70	0.70811
75	0.69130

On comparing Table VI with Table II it will be seen that it agrees most closely with the first scale, which is Francœur's, and which has been adopted by the United States Petroleum Association.

CONCLUSION.

In conclusion I would suggest to the Academy that, owing to the very extensive use which is made of the Baumé instruments, it would be eminently proper to consider the propriety of legislation on the part of Congress, or some other means, for establishing a fixed value to the two scales of the Baumé instruments, and I will offer at the proper time the following resolution:

"*Resolved*, That a committee be appointed to consider what action, if any, is desirable, with a view to establishing a legal value for the degrees of the Baumé and other hydrometers of arbitrary scales; the committee to report at the next meeting."

NOTE.—This resolution was adopted, and the following committee was appointed: Julius E. Hilgard, Superintendent United States Coast Survey, Washington, D. C.; Henry Morton, President Stevens Institute, Hoboken, N. J.; C. F. Chandler, Professor of Chemistry, Columbia College, New York.

I would further state that I am very largely indebted to my assistant, F. G. Wiechmann, Ph.B., for the experimental and historical data contained in the preceding tables.

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ON SMALL DIFFERENCES OF SENSATION.

ON SMALL DIFFERENCES OF SENSATION.

READ OCTOBER 17, 1884.

By C. S. PEIRCE and J. JASTROW.

The physiological psychologists assume that two nerve excitations alike in quality will only produce distinguishable sensations provided they differ in intensity by an amount greater than a fixed ratio. The least perceptible difference of the excitations divided by half their sum is what they call the *Unterschiedsschwelle.* Fechner* gives an experiment to prove the fact assumed, namely: He finds that two very dim lights placed nearly in line with the edge of an opaque body show but one shadow of the edge. It will be found, however, that this phenomenon is not a clearly marked one, unless the lights are nearly in range. If the experiment is performed with lateral shifting of one of the lights, and with a knowledge of the effects of a telescope upon the appearance of terrestrial objects at night, it will be found very far from conclusive.

The conception of the psychologists is certainly a difficult one to seize. According to their own doctrine, in which the observed facts seem fully to bear them out, the intensity of the sensation increases continuously with the excitation, so that the least increase of the latter must produce a corresponding increase of the former. And, indeed, the hypothesis that a continuous increase of the excitation would be accompanied by successive discrete increments of the sensation, gratuitous as it would be, would not be sufficient to account for a constant *Unterschiedsschwelle.* We are therefore forced to conclude that if there be such a phenomenon it has its origin, not in the faculty of sensation, but in that of comparing sensations. In short, if the phenomenon were established, we should be forced to say that there was a least perceptible difference of sensation—a difference which, though existing in sensation, could not be brought into consciousness by any effort of attention. But the errors of our judgments in comparing our sensations seem sufficiently accounted for by the slow and doubtless complicated process by which the impression is conveyed from the periphery to the brain; for this must be liable to more or less accidental derangement at every step of its progress. Accordingly we find that the frequencies of errors of different magnitudes follow the probability curve, which is the law of an effect brought about by the sum of an infinite number of infinitesimal causes. This theory, however, does not admit of an *Unterschiedsschwelle.* On the contrary, it leads to the method of least squares, according to which the multiplication of observations will indefinitely reduce the error of their mean, so that if of two excitations one were ever so little the more intense, in the long run it would be judged to be the more intense the majority of times. It is true that the astronomers themselves have not usually supposed that this would be the case, because (apart from constant errors, which have no relevancy to the present question) they have supposed this extreme result to be contrary to common sense. But it has seemed to us that the most satisfactory course would be to subject the question to the test of direct experiment. If there be a least perceptible difference, then when two excitations differing by less than this are presented to us, and we are asked to judge which is the greater, we ought to answer wrong as often as right in the long run. Whereas, if the theory of least squares is correct, we not

*Elemente der Psychophysik, I, p. 242.

only ought to answer right oftener than wrong, but we ought to do so in a predictible ratio of cases.*

We have experimented with the pressure sense, observing the proportion of errors among judgments as to which is the greater of two pressures, when it is known that the two are two stated pressures, and the question presented for the decision of the observer is, which is which? From the probability, thus ascertained, of committing an error of a given magnitude, the probable error of a judgment can be calculated according to the mathematical theory of errors. If, now, we find that when the ratio of the two pressures is smaller than a certain ratio, the erroneous judgments number one-half of the whole, while the mathematical theory requires them to be sensibly fewer, then this theory is plainly disproved, and the maximum ratio at which this phenomenon is observed the so-called *Unterschiedsschwelle.* If, on the other hand, the values obtained for the probable error are the same for errors varying from three times to one-fourth of the probable error (the smallest for which it is easy to collect sufficient observations), then the theory of the method of least squares is shown to hold good within those limits, the presumption will be that it extends still further, and it is possible that it holds for the smallest differences of excitation. But, further, if this law is shown to hold good for difference so slight that the observer is not conscious of being able to discriminate between them at all, all reason for believing in an *Unterschiedsschwelle* is destroyed. The mathematical theory has the advantage of yielding conceptions of greater definiteness than that of the physiologists, and will thus tend to improve methods of observation. Moreover, it affords a ready method for determining the sensibility or fineness of perception and allows of a comparison with the results of others; for, knowing the number of errors in a certain number of experiments, and accepting the conclusions of this paper, the calculated ratio to the total excitation of that variation of excitation, in judging which we should err one time out of four, measures the sensibility. Incidentally our experiments will afford additional information upon the value of the normal average sensibility for the pressure sense, which they seem to make a finer sense than it has hitherto been believed to be. But in this regard two things have to be noted: (1) Our value relates to the probable error or the value for the point at which an error is committed half the time; (2) in our experiments there were two opportunities for judging, for the initial weight was either first increased and then diminished, or *vice versa*, the subject having to say which of these two double changes was made. It would seem at first blush that the value thus obtained ought to be multiplied by $\sqrt{2}$ (1.414) to get the error of a single judgment. Yet this would hardly be correct, because the judgment, in point of fact, depended almost exclusively on the sensation of increase of pressure, the decrease being felt very much less. The ratio $\sqrt{2}$ (1.414) would therefore be too great, and 1.2 would perhaps be about correct. The advantage of having two changes in one experiment consists in this: If only one change were employed, then some of the experiments would have an increase of excitation only and the others a decrease only; and since the former would yield a far greater amount of sensation than the latter, the nature of the results would be greatly complicated; but when each experiment embraces a

* The rule for finding this ratio is as follows: Divide the logarithm of the ratio of excitations by the probable error and multiply the quotient by 0.477. Call this product t. Enter it in the table of the integral θt, given in most works on probabilities; θt is the proportion of cases in which the error will be less than the difference between the given excitations. In all these cases, of course, we shall answer correctly, and also by chance in one-half of the remaining cases. The proportion of erroneous answers is therefore $(1-\theta t)\div 2$. In the following table the first column gives the quotient of the logarithm of the ratio of excitation, divided by the probable error, and the second column shows the proportion of erroneous judgments:

0.0	0.50
0.05	0.49
0.1	0.47
0.25	0.43
0.5	0.37
1.0	0.25

To guess the correct card out of a pack of fifty-two once in eleven times it would be necessary to have a sensation amounting to 0.37 of the probable error. This would be a sensation of which we should probably never become aware, as will appear below.

double change this difference in the amount of sensation caused by an increase and decrease of pressure affects every experiment alike, and the liability to error is constant.*

Throughout our observations we noted the degree of confidence with which the observer gave his judgment upon a scale of four degrees, as follows:

0 denoted absence of any preference for one answer over its opposite, so that it seemed nonsensical to answer at all.

1 denoted a distinct leaning to one alternative.

2 denoted some little confidence of being right.

3 denoted as strong a confidence as one would have about such sensations.

We do not mean to say that when zero was the recorded confidence, there was absolutely no sensation of preference for the answer given. We only mean that there was no sensation that the observer noticed when attending to his feelings of this sort as closely as he conveniently could, namely, closely enough to mark them on this scale. The scale of confidence fluctuated considerably. Thus, when Mr. Jastrow passed from experiments upon differences of weight of 60, 30, and 15 on the thousand to differences of 20, 10, and 5 on the thousand, although the accuracy of his judgments was decidedly improved, his confidence fell off very greatly, owing to his no longer having the sensation produced by a difference of 60 present to his memory. The estimations of confidence were also rough, and might be improved in future work. The average marks seem to conform to the formula—

$$m=c\log\frac{p}{1-p}$$

where m denotes the degree of confidence on the scale, p denotes the probability of the answer being right, and c is a constant which may be called the index of confidence.

To show that this formula approximates to the truth, we compare it with the average marks assigned to estimates of differences for which more than a hundred experiments were made. Mr. Jastrow's experiments are separated into groups, which will be explained below.

First group.

Ratio of pressures.	Peirce, observer.		Jastrow, observer.			
	c=1.25.		c=1.5.		c=0.0.	
	Mean confidence.		Mean confidence.		Mean confidence.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
1.015	0.14	0.10	0.30	0.2	0.34	0.27
1.030	0.30	0.35	0.40	0.42	0.55	0.56
1.060	0.70	0.70	0.85	0.87	1.02	1.22

Ratio of pressures.	Jastrow, observer.			
	c=0.25.		c=0.4.	
	Mean confidence.		Mean confidence.	
	Observed.	Calculated.	Observed.	Calculated.
1.005	0.00	0.03	0.00	0.06
1.010	0.07	0.06	0.05	0.12
1.020	0.12	0.12	0.50	0.39

* The number of errors, when an increase of weight was followed by a decrease, was slightly less than when the first change was a decrease of pressure.

The judgments enunciated with any given degree of confidence were more likely to be right with greater differences than with smaller differences. To show this, we give the frequency of the different marks in Mr. Jastrow's second, third, and fourth groups.*

The apparatus used was an adaptation of a "Fairbanks" post-office scale; upon the end of the beam of which was fixed a square enlargement (about one-half inch square), with a flat top, which served to convey the pressure to the finger in a manner to be presently described. This was tightly covered with an India-rubber cap, to prevent sensations of cold, &c., from contact with the metal. A kilogram placed in the pan of the balance brought a pressure of one-fourth

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* The result of our observations on the confidence connected with the judgments is as follows:

[Subject, Mr Peirce.]

Variations.	Average confidence.	Number of sets of 50.
Grams.		
60	.67	7
30	.28	6
15	.15	5

[Subject, Mr. Jastrow.]

Variations.	Average confidence.	Number of sets of 50.
60	.90	13
30	.51	12
15	.30	12
20	.11	12
10	.06	12
5	.00	10

In 1,125 experiments (subject, Mr. Peirce)—variations 15, 30, and 60 grams—there occurred confidence of 3, 35 times (3 per cent.); of 2, 102 times (9 per cent.); of 1, 282 times (25 per cent.); of 0, 706 times (63 per cent.). In these experiments there were 332 (29 per cent.) errors committed, of which 1 (0.3 per cent.) was made in connection with a confidence 3; 10 (3 per cent.) with a confidence 2; 51 (15 per cent.) with a confidence 1; 270 (81 per cent.) with a confidence 0. From which we find that in connection with a confidence of 3 there occurred 1 error in 35 cases (3 per cent.); with a confidence of 2, 10 errors in 102 cases (10 per cent.); with a confidence of 1, 51 errors in 282 cases (18 per cent.); with a confidence of 0, 270 errors in 706 cases (38 per cent.).

In 1,975 experiments (subject, Mr. Jastrow)—variations 15, 30, and 60 grams—there occurred confidence of 3, 62 times (3 per cent.); of 2, 196 times (10 per cent.); of 1, 594 times (30 per cent.); of 0, 1,123 times (57 per cent.). In these experiments there were 451 (23 per cent.) errors committed, of which 2 (0.4 per cent.) were made in connection with a confidence of 3; 12 (3 per cent.) with a confidence of 2; 97 (22 per cent.) with a confidence of 1, 340 (75 per cent.) with a confidence of 0. Again, in connection with a confidence of 3, errors occurred twice in 62 cases (3 per cent.); with a confidence of 2, 12 times in 196 cases (6 per cent.); with a confidence of 1, 97 times in 504 cases (16 per cent.); with a confidence of 0, 340 times in 1,123 cases (30 per cent.).

In 1,675 experiments (subject, Mr. Jastrow)—variations 5, 10, and 20 grams—there occurred confidences of 3, none; of 2, none; of 1, 115 times (7 per cent.); of 0, 1,560 times (93 per cent.). In these experiments there were 538 (32 per cent.) errors committed, of which 16 (3 per cent.) occurred in connection with a confidence of 1; 522 (97 per cent.) with a confidence of 0. Again, in connection with a confidence of 1, errors occurred 16 times in 115 cases (14 per cent.); with a confidence of 0, 522 times in 1,560 cases (34 per cent.).

Second group.

Ratio of weights.	Mark 0.	Mark 1.	Mark 2.	Mark 3.
1.015	110 right 66 wrong	51 right 17 wrong	3 right 2 wrong	1 right 0 wrong
1.030	106 right 35 wrong	72 right 11 wrong	23 right 1 wrong	2 right 0 wrong
1.060	86 right 8 wrong	75 right 1 wrong	54 right 2 wrong	24 right 0 wrong

of its weight upon the finger. The differential pressure was produced by lowering upon the pan of the balance a smaller pan into which the proper weights could be firmly fixed; this little pan had its bottom of cork, and was placed upon a piece of flannel which constantly remained in the pan of the balance. It was lifted off and on by means of a fine India-rubber thread, which was so much stretched by the weight as certainly to avoid any noise or jar from the momentum of the descending pan. A sufficient weight could also be hung on the beam of the balance, so as to take off the entire pressure from the finger at the end of each experiment. This weight could be applied or removed by means of a cam acting upon a lever; and its bearings upon the beam were guarded by India-rubber. It was found that the use of this arrangement, which removed all annoying irregularities of sensation connected with the removal and replacement of the greater (initial) pressure, rendered the results more uniform and diminished the probable error. It also shortened the time necessary for performing the experiments, so that a series of 25 experiments was concluded before the effects of fatigue were noticeable. It may be mentioned that certain causes tended to the constant decrease of the probable error as the experiments went on, these mainly being an increased skill on the part of the *operator* and an education of the sensibility of the *subject*. The finger was supported in such a way as to be lightly but firmly held in position, all the muscles of the arm being relaxed; and the India-rubber top of the brass enlargement at the end of the beam of the balance was never actually separated from the finger. The projecting arm of a filter-stand (the height of which could be adjusted) with some attachments not necessary to detail, gently prevented the finger from moving upwards under the pressure exerted by the weight in the pan. In the case of Mr. Peirce as subject (it may be noted that Mr. Peirce is left-handed, while Mr. Jastrow is strongly right-handed) the tip of forefinger, and in the case of Mr. Jastrow of the middle finger, of the left hand were used. In addition, a screen served to prevent the subject from having any indications whatever of the movements of the operator. It is hardly necessary to say that we were fully on our guard against unconsciously received indications.

The observations were conducted in the following manner: At each sitting three differential weights were employed. At first we always began and ended with the heaviest, but at a later period the plan was to begin on alternate days with the lightest and heaviest. When we began with the heaviest 25 observations* were made with that; then 25 with the middle one, and then 25 with the lightest; this constituted one-half of the sitting. It was completed by three more sets of 25, the order of the weights being reversed. When we began with the lightest the heaviest was used for the third and fourth sets. In this way 150 experiments on each of us were taken at one sitting of two hours.

A pack of 25 cards were taken, 12 red and 13 black, or *vice versa*, so that in the 50 experiments made at one sitting with a given differential weight, 25 red and 25 black cards should be used. These cards were cut exactly square and their corners were distinguished by holes punched in them so as to indicate the scale of numbers (0, 1, 2, 3) used to designate the degree of confidence of the judgment. The backs of these cards were distinguished from their faces. They were, in fact, made of ordinary playing-cards. At the beginning of a set of 25, the pack was well shuffled, and, the operator and subject having taken their places, the operator was governed by the color

Third and fourth groups.

[Marks 2 and 3 do not occur.]

Ratio of weights.	Mark 0.	Mark 1.
1.005	294 right	2 right
	203 wrong	1 wrong
1.010	366 right	32 right
	192 wrong	30 wrong
1.020	395 right	68 right
	131 wrong	6 wrong

* At first a short pause was made in the set of 25, at the option of the subject; later this was dispensed with.

of the successive cards in choosing whether he should first diminish the weight and then increase it, or *vice versa*. If the weight was to be first increased and then diminished the operator brought the pressure exerted by the kilogram alone upon the finger of the subject by means of the lever and cam mentioned above, and when the subject said "change" he gently lowered the differential weight, resting in the small pan, upon the pan of the balance. The subject, having appreciated the sensation, again said "change," whereupon the operator removed the differential weight. If, on the other hand, the color of the card directed the weight to be first diminished and then increased, the operator had the differential weight already on the pan of the balance before the pressure was brought to bear on the finger, and made the reverse changes at the command of the subject. The subject then stated his judgment and also his degree of confidence, whereupon the total pressure was at once removed by the cam, and the card that had been used to direct the change was placed face down or face up according as the answer was right or wrong, and with corner indicating the degree of confidence in a determinate position. By means of these trifling devices the important object of rapidity was secured, and any possible psychological guessing of what change the operator was likely to select was avoided. A slight disadvantage in this mode of proceeding arises from the long runs of one particular kind of change, which would occasionally be produced by chance and would tend to confuse the mind of the subject. But it seems clear that this disadvantage was less than that which would have been occasioned by his knowing that there would be no such long runs if any means had been taken to prevent them. At the end of each set the results were of course entered into a book.*

The following tables show the results of the observations for each day:

Date.	Ratios of pressures. [Subject: Mr. Peirce.]						
	1.100	1.080	1.060	1.050	1.040	1.030	1.015
December 10	2 errors.	....	....	13 errors.	....	....	....
December 13	....	4 errors.	8 errors.	....	15 errors.	....	....
December 17	....	....	11	....	....	20 errors.	....
December 20	....	....	7	....	....	16	21 errors.
January 3	....	....	14	....	....	20	28
January 15	....	....	15	....	....	29	28
January 22	....	....	12	....	....	16	20
January 24	....	....	6	....	....	15	22
Means	2	4	10.4±1.0	13	15	19.3±1.4	21.6±1.1
Calculated from probable error=0.051	4.6±1.0	7.2±1.6	10.7±0.8	12.7±2.1	14.9±2.2	17.2±0.9	21.0±1.1
Average confidence.							
Observed	1.9	0.9	0.7	0.8	0.3	0.3	0.2
Calculated	1.3	1.0	0.7	0.6	0.5	0.3	0.2

The numbers in the columns show the number of errors in fifty experiments. With the average number of errors in a set of fifty we compare the theoretical value of this average as calculated by the method of least squares. The number .051 thus obtained in this case best satisfies the mean number of errors. The numbers affixed with a sign denote, in the upper row the observed (*a posteriori*) probable error of the mean value as given, in the lower row the calculated (*a priori*) probable error. The last two lines give the average confidence observed and calculated with each variation of the ratios of pressure. It will be seen that the correspondence between the real and theoretical numbers is close, and closest when the number of sets is large. The probable errors also closely correspond, the observed being, as is natural, slightly larger than the calculated probable errors.

* In the experiments of December, 1883, and January, 1884, the method as above described was not fully perfected the most important fault being that the total weight instead of being removed and replaced by a mechanical device, was taken off by the operator pressing with his finger upon the beam of the balance.

The following is a similar table for Mr. Jastrow as subject:

Date.	Ratios of pressures.									
	1.100	1.080	1.060	1.050	1.040	1.030	1.020	1.015	1.010	1.005
December 10	5			19						
December 13		9	15		15					
December 17			14			23				
December 20			10			17		25		
January 3			8			14		24		
January 10			7			13		17		
January 15			12			6		22		
January 22			11			10		16		
January 24			4			11		18		
February 11			1			7		18		
February 17			2			10		17		
February 18			2			11		17		
February 24			2			8		15		
March 4							13		16	
March 5							13		17	
March 18							14		19	29
March 19							11		21	18
March 23							14		17	18
March 25							12		16	18
March 30							11		16	21
March 31							10		15	21
April 2							11		17	21
April 3							9		18	20
April 6							12		15	21
April 7			0			5	7	14	15	17
Means	5	9	6.6	19	15.0	11.6	11.4	18.9	16.8	20.5

It would obviously be unfair to compare these numbers with any set of theoretical numbers, since the probable error is on the decrease throughout, owing to effects of practice, etc. For various reasons we can conveniently group these experiments into four groups. The first will include the experiments from December 10 to January 22, inclusive; the second from January 24 to February 24, inclusive; the third from March 4 to March 25, inclusive; the fourth from March 30 to the end of the work.

The mean results for the different groups are exhibited in the following tables:

First group.

[Probable error=0.05.]

Ratios of pressures.	Number of sets of 50.	Average number of errors.		Average confidence.	
		Observed.	Calculated from probable error.	Observed.	Calculated.
1.100	1	5	4.4±1.4	0.9	1.5
1.080	1	9	7.0±1.7	0.9	1.2
1.060	7	11.0±0.7	10.4±0.7	0.85	0.9
1.050	1	19	12.5±2.1	0.35	0.7
1.040	1	15	14.7±2.2	0.3	0.6
1.030	6	13.8±1.5	17.0±0.9	0.5	0.4
1.015	5	20.8±1.1	21.0±1.1	0.3	0.2

Second group.

[Probable error=0.0235.]

1.060	5	2.2±0.3	2.1±0.4	1.0	1.2
1.030	5	9.4±0.6	9.6±0.8	0.55	0.6
1.015	5	17.0±0.3	16.6±1.0	0.3	0.3

Third group.

[Probable error=0.02.]

Ratios of pressures.	Number of sets of 50.	Average number of errors.		Average confidence.	
		Observed.	Calculated from probable error.	Observed.	Calculated.
1.020	6	12.8±0.3	12.5±0.8	0.12	0.12
1.010	6	17.7±0.6	18.3±0.9	0.07	0.06
1.005	4	20.7±1.7	21.6±1.2	0.00	0.03

Fourth group.

[Probable error=0.0155.]

1.060	1	0	0.8±0.6	1.6	....
1.030	1	5	4.8±1.4	0.5	0.4
1.020	6	10.0±0.5	9.6±0.8	0.1	0.2
1.015	1	14	12.8±2.1	0.1	0.13
1.010	6	16	16.5±0.9	0.05	0.12
1.005	6	20.8±0.4	20.6±1.0	0.00	0.06

The tables show that the numbers of errors follow, as far as we can conveniently trace them, the numbers assigned by the probability curve,* and therefore destroy all presumption in favor of an *Unterschiedsschwelle.* The introduction and retention of this false notion can only confuse thought, while the conception of the mathematician must exercise a favorable influence on psychological experimentation.†

The quantity which we have called the degree of confidence was probably the secondary sensation of a difference between the primary sensations compared. The evidence of our experiments

*In the tables of the third and fourth groups, there is a marked divergence between the *a priori* and *a posteriori* probable error, for the average number of errors in 50, making the observed probable error too small. This can only be partly accounted for by the fact that the subject formed the unconscious habit of retaining the number of each kind of experiment in a set and answering according to that knowledge. In point of fact the plus errors and minus errors separately do not exhibit the singular uniformity of their sums, for which we are quite unable to account. Thus in the fourth group we have:

Number of + and — errors.

Date.	1.020	1.010	1.005
March 30..........	—4, + 7	—6, +10	—13, + 8
March 31..........	—7, + 3	—5, +10	— 6, +15
April 2..........	—1, +10	—8, + 9	— 8, +13
April 3..........	—4, + 5	—4, +14	—10, +10
April 6..........	—6, + 6	—8, + 7	—10, +11
April 7..........	—5, + 9	—8, + 7	— 8, + 9

†The conclusions of this paper are strengthened by the results of a series of experiments on the color sense, made with the use of a photometer by Mr. Jastrow. The object was to determine the number of errors of a given magnitude, and compare the numbers thus ascertained with the theoretical numbers given by the probability curve. A thousand experiments were made. Dividing the magnitude of the errors from 0 to the largest error, made into 5 parts, the number of errors, as observed and calculated, that occur in each part are as follows:

Observed......	199	181	217	213	190
Calculated	213	197	209	181	200

These numbers would be in closer accordance if the probable error were the same throughout, as it is not owing to the effects of practice, &c. Moreover, the experiments were made on different colors—300 on white and 100 each on yellow, blue, dove, pink, green, orange, and brown. These experiments were not continuous.

seems clearly to be that this sensation has no *Schwelle*, and vanishes only when the difference to which it refers vanishes. At the same time we found the subject often overlooked this element of his field of sensation, although his attention was directed with a certain strength toward it, so that he marked his confidence as *zero*. This happened in cases where the judgments were so much affected by the difference of pressures as to be correct three times out of five. The general fact has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another's minds in large measure from sensations so faint that we are not fairly aware of having them, and can give no account of how we reach our conclusions about such matters. The insight of females as well as certain "telepathic" phenomena may be explained in this way. Such faint sensations ought to be fully studied by the psychologist and assiduously cultivated by every man.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SIXTH MEMOIR.

DESCRIPTION OF AN ARTICULATE OF DOUBTFUL RELATIONSHIP FROM THE TERTIARY BEDS OF FLORISSANT, COLO.

DESCRIPTION OF AN ARTICULATE OF DOUBTFUL RELATIONSHIP FROM THE TERTIARY BEDS OF FLORISSANT, COLORADO.

READ AT WASHINGTON, APRIL 20, 1882.

BY SAMUEL H. SCUDDER.

Among the remains of animals in my hands found in the ancient lake basin of Florissant are about forty specimens of an onisciform arthropod, about a centimeter in length, whose affinities have proved very perplexing. This does not result from poorness of preservation, for among the numerous specimens apparently all the prominent external features are found completely preserved, and even the course of some of the internal organs may occasionally be traced; but it presents such anomalies of structure that we are at a loss where to look for its nearest kin.

It appears to be an aquatic animal. Its body consists of three large subequal thoracic joints, and an abdomen about half as large again as any one of them, with occasional indications of a feeble division into four segments. These are the only jointed divisions that can be found in the body, there being no distinct head. The thoracic segments are so considered because each bears a pair of legs, which occur nowhere else. Their dorsal plates are large, flat longitudinally, and arched transversely; smooth, and deeply and narrowly notched in the middle of the front margin.

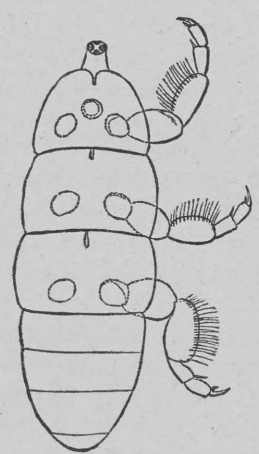

Fig. 1.

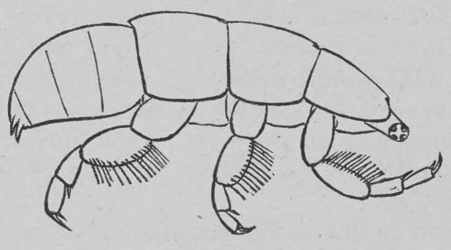

Fig. 2.

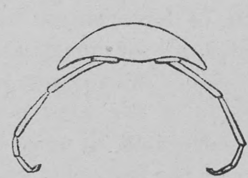

Fig. 3.

Fig. 1, dorsal view; fig, 2, lateral view; fig. 3, transverse sectional view of *Planocephalus aselloides* from the oligocene of Florissant, Colorado, restored, and magnified about six diameters.

The first plate, in which the median notch is more conspicuous and open than in the others, also narrows and becomes more arched in front, so as to form a sort of hood. The legs are very broad and compressed, and adapted to swimming, which was apparently their use, as there would be no need of such compression to crawl into chinks when the body is so much arched. They consist of a femur, tibia, and two tarsal joints, terminated by a single curved claw. The femur is very large, subovate, inserted (presumably by a coxa) in large cavities, those of opposite sides separated by their own width, and situated a little behind the middle of each segment. The tibia is also very large and subovate, but more elongated and squarer at the ends, being about twice as long as broad, and fringed on the anterior edge by a row of delicate hairs as long as the width of the joint. Of the two tarsal joints, the basal is a little the larger, being both longer and stouter. Each is armed at the tip internally with a tolerably stout spine of moderate length, and together they are a little longer than the tibia, much slenderer, and quadrate in form. The terminal claw is about half as long as the terminal joint. The hind legs are somewhat stouter and the middle pair a little shorter than the others; but otherwise they closely resemble each other.

The different segments of the thorax, as stated, are protected above by the development of distinct chitinous plates, the lower edges of which are clearly marked, and extend downward to the concealment, on a side view, of the lower part of the body. The abdomen, however, seems to have no such specialization of the integument of the upper surface. It is stout, apparently well rounded transversely, and tapers to a produced but blunt tip, which is armed with a pair of slightly recurved stout claws, two or three times longer than the leg-claws, arranged as if to drag the body backward. The abdomen is faintly divided into four segments, often entirely obscured. Of these the terminal usually appears shorter than the others, which are subequal.

These divisions of the body are all that appear to have belonged to the animal; and it is the most remarkable fact in its organization that it certainly had no distinct chitinous head. This is the more surprising from the clearness with which the thoracic segments are marked. All that one can find preserved is what appears to be a ring of buccal plates terminating anteriorly the alimentary canal, and which was evidently capable of being thrust forward a long distance beyond the body. If it were not for the unusual preservation of the alimentary canal we should be forced to consider the head as lost from all the specimens, notwithstanding the nearly perfect preservation of the other parts; but in several specimens the alimentary tube can be traced with ease half through the body, terminating in front in these more or less clearly preserved chitinous plates, arranged to form a circle a little smaller than the coxal cavities. What is most remarkable is the extension of this alimentary tube and accompanying buccal plates like a proboscis far beyond the limits of the body; sometimes forward (apparently through the anterior notch) to a distance in front of the first segment equal to half the length of the latter; more often directed downward as well as outward, perhaps between the front legs, and occasionally extending beyond the body to nearly or quite *the entire length of the same.* It seems to leave its direct course within the body at about the middle of the first thoracic segment, directly in front of which position the buccal plates appear in one or two specimens, apparently in the position of repose. The various positions in which these buccal plates are found outside the body, both when their connection with the tube is traceable and when it is obscure or fails, shows how perfectly movable a proboscis the creature possessed. The external parts of the head, then, may be said to have probably been composed entirely of a flexible, extensible membrane capable of protrusion as a fleshy proboscis, separated by no line of demarkation from the first thoracic segment, and bearing as appendages only a series of buccal plates for mouth-parts, and beyond this nothing—neither cranium, eyes, antennæ, nor palpi. In the absence of eyes, one would naturally look for the development of tactile organs of some sort; but nothing of the kind is discoverable on the most careful special search, unless such an office may be performed by long delicate hairs which seem, in some few instances, to be scattered distantly over the projected mouth-tube.

A special study of the buccal plates in the twenty-four or twenty-five specimens which best show them gives no very satisfactory explanation of their form and relations. They have been said to form a ring, because in a considerable number they are so arranged; but it may be doubted whether this appearance is not due to the flaking of the chitinous parts. Like the lips of the notches of the thoracic segments, the buccal apparatus was evidently more dense and thicker than other tegumentary parts, for these are darker colored than the other parts and often carbonaceous In this condition the central portions seem liable to flake away and leave the thinner edges with ragged fragments of the carbonaceous inner portions attached, thus frequently forming a sort of irregular ring of dark chitine. On the other hand, it is just as common for fragments to become chipped out from the edges, or for rounded bits to fall out here and there, producing thereby an almost endless variety of present appearances. Among these it is difficult to trace the clew to the original arrangement and form of the plates. One might anticipate that these would have occurred around the central orifice of a proboscis; and if anything of this sort was present it would appear the most probable (though extremely doubtful) that there were four subtriangular plates of pretty large size, the lateral the larger, nearly meeting by their tips at the center. From specimens, however, which are least broken, it would seem quite as probable that the apparatus consisted of two attingent or overlapping circular plates, placed transversely, densest centrally, which by their consolidation form an oval rounded mass. How such a pair of plates, or compound plates, could have subserved any purpose in the procuring of food, I cannot understand, but that such is their not unfrequent appearance, especially when seen through and protected by the thoracic shield of the first segment, is nevertheless the fact. It is to be hoped that other specimens may set this matter at rest. Those at hand allow no more definite statement than has been made. About three-fourths of the specimens of this species show the buccal plates more or less distinctly. In all but three they lie outside the body, usually at a distance from it of about half the length of the first thoracic segment. In a fourth specimen they lie half protruding at the front edge of the body.

These buccal plates, as already stated, are the only hard parts of the head, and the only appendages. Indeed, the only claim this portion of the body has to be called the head at all is that it is certainly the anterior extremity of the digestive canal. On account of this peculiarity of the organization of the head, the creature, which is certainly widely different from anything known, may be called *Planocephalus* (πλανάω, κεφαλή), and on account of its denisciform body, *Planocephalus aselloides*.

The first impression the sight of this strange headless creature conveys is that of an isopod crustacean. But the limited number of legs at once puts its reference to the Crustacea out of question, since no abdominal legs at all are present. Even in the parasitic Crustacea, where some of the legs are aborted, the same is the case with the segments themselves and with the joints of the legs which remain. The clear distinction which obtains between the thoracic and abdominal regions, and the limitation of the jointed legs to a single pair on each thoracic segment seems to lead one strongly to the conviction that these important elements of its construction place it among insects. The structure of the legs and the small tapering abdomen furnished with small anal appendages tend to the same conclusion.

Where among insects it should be placed is more questionable. Thinking it possibly a larval form, careful search has been made among all the groups into which it could by any possibility be presumed to fall, viz, among the Neuroptera and Coleoptera, but nothing in the slightest degree seeming to be related to it could be found, and its conspicuous size rendered it the less probable that a kindred form would be overlooked. On account, however, of its apterous character, and the discovery in recent years of certain curious types of animals (all of them, however, very minute) whose affinities have provoked more than usual discussion, my attention was early drawn toward certain resemblances which Planocephalus bears to the Pauropidæ among Myriapods and to the Thysanura, and here, if anywhere, its affinities seem likely to be found.

Its passing resemblance to the obtected forms of Pauropoda which Ryder has published under the name of Eurypauropodidæ is certainly very considerable, especially when it is remembered that the young of Pauropoda bear only three pairs of legs. The position of the more mobile part of the head of Eurypauropus beneath the cephalic shield is the same that the head of Planocephalus bears to the first thoracic shield; and the mouth-parts in both are confined to a somewhat similar circular area; there are no eyes in either, and the legs terminate in a single curved claw.

On the other hand, not only are antennæ of a highly organized character developed in Pauropoda, but the upper portion of the head carries a cephalic shield as large and conspicuous as the others; two pairs of legs are developed in the adult on every or nearly every segment of the body, and always on the abdominal to the same extent as on the thoracic segments, no abdomen being distinct from a thorax as in Planocephalus, but all the joints of the body entirely similar; the legs of the Pauropoda are formed on the myriapodal type, consisting of cylindrical undifferentiated joints, while those of Planocephalus are hexapodal in character, having a clearly defined femur and tibia, and a two-jointed tarsus conspicuously smaller and shorter than the preceding joints, of different form and apically spined.

The closer, therefore, we compare these two types the less important seem the points of resemblance, and the more important the points of divergence between them; for in the clear distinction of the thorax and abdomen, the absence of abdominal legs, and the structure of the legs themselves—fundamental features of its organization—Planocephalus clearly belongs to the true hexapod type of insects.

Its probable reference to the Thysanura may be defended on both negative and positive grounds. There is no other group of hexapods to which it could be considered as more likely to belong, and there are some special thysanuran features in its structure, anomalous as it is. Since Packard has shown the reasonableness of placing the Symphyla (=Scolopendrella) of Ryder in the Thysanura, with the Collembola and Cinura as co-ordinate groups, the range of the Thysanura has been extended, and as a group of equivalent taxonomic value to the larger divisions of winged insects it has seemed itself to gain a better *ratio vivendi.* It is not necessary, therefore, in considering the relations of Planocephalus to Thysanura as a whole, to limit ourselves to points of comparison which it may have to one or another of its subordinate groups, but consider any points of resemblance we may find to any of these groups indifferently. The thoracic segments remind us not a little of some Cinura, while the abdomen as a whole recalls many of the Collembola, its approximated pair of specialized anal appendages being also like the variously developed organs of all Thysanura and unlike anything we can recall in any myriapod. The legs, in the development of the basal joints and in the smaller double-jointed tarsus, are closely related to those of some Cinura—built indeed upon the same general pattern, excepting that in Planocephalus they are specially developed for swimming. In the claw of our fossil genus we have something decidedly thysanuriform. We have heretofore spoken of the two tarsal joints as each armed apically with an interior spine; but that of the final joint arises from the base of the curving claw, and takes on more or less its direction, though only half as long as it, causing it to resemble very closely the smaller digit of the claw of both Collembola and Cinura, which is always inferior to the larger, and not infrequently, as in Lepidocyrtus, etc., straight instead of curved.

Of course, the rudimentary character of the head and the entire obliteration of the cephalic plates renders our fossil very distinct from any known type of Thysanura. But these features separate it quite as widely from any other group that may be suggested for it, and taking into account the considerable development of the thoracic portions, we must look upon Planocephalus as in some sense a degraded form, descended from a type in which the head was developed at least to some extent; and this renders it more probable that we have here found its proper place. Moreover when we examine the mouth-parts of Podura, we find them partially withdrawn within the head, reduced in external presentation to a small circle at the end of a conical protrusion of the under side of the head. Take away the cephalic plates, withdraw the mouth-parts to the same protection of the first thoracic segment which they now enjoy under the cephalic dome, imagine further that the mouth-parts could be protruded to their original position when covered by a cephalic shield, and we have about the same condition of things we find in Planocephalus; indeed the extensibility of the mouth parts beyond the thoracic shield seems quite what one might expect after the loss of the hard parts of the head; and the mouth-parts of Planocephalus bear much the same relative position to the first thoracic shield which those of Podura bear to the cephalic shield.

Assuming, then, that Planocephalus is a true hexapod, its general relations are certainly with the Thysanura rather than with any other group; while the character of the legs, the half developed double claw, and the anal appendages specialized to peculiar use are characters which are positively thysanuran. Add to this that we find in Podura something in a remote degree analogous to the extraordinary mouth-parts of Planocephalus, which we should in vain seek elsewhere, and the probability that we find here its nearest allies is rendered very strong; and the more so from the diversity of form and type in this group since the addition to it of Scolopendrella. The discovery of a collophore or something homologous to it would, we conceive, be decisive on the point; but the lateral preservation of nearly all the specimens of this fossil, and the obscurity of the base of the abdomen in nearly all, not only forbid its determination in those yet found, but render it doubtful if it will ever be discovered.

The position of this group among the Thysanura must be an independent one, between the Cinura and the Symphyla, and of an equivalent value to them. For such a group the name of BALLOSTOMA is proposed, in reference to the remarkable power it possessed of thrusting forward the gullet and mouth-parts. It would be characterized by the peculiarity named, by the lack of any chitinous frame-work of the head, the equal development of three thoracic segments developed dorsally as shields, and all separated from a cylindrical abdomen, which is armed at tip with a pair of hooks for crawling; legs largely developed and with expanded and flattened femora and tibiæ, the tarsi two jointed. The principal points toward which attention should be directed for the more perfect elucidation of its structure are the buccal plates and a possible collophore.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SEVENTH MEMOIR.

THE STRUCTURE OF THE COLUMELLA AURIS IN THE PELYCOSAURIA.

THE STRUCTURE OF THE COLUMELLA AURIS IN THE PELYCOSAURIA

READ OCTOBER 16, 1884.

By E. D. Cope.

In a specimen of the Permian reptile *Clepsydrops leptocephalus* Cope,* the columella auris was found nearly in its normal position. It was found lying on the internal side of the normally joined squamosal and quadrate bones, the greater part of it within the former, but the distal extremity overlapping the superior part of the latter. These elements have lost their attachment to the cranium proper, so that the connection of the columella with the latter is not visible.

The columella is of unusual size as compared with other bones of the skull. Thus while the vertical length of the premaxillary bone is M. .060, and its width at the third tooth is .022, and while the vertical length of the quadrate bone is .085, the dimensions of the columella auris are as follows:

Length on inside of curve		.072
Greatest diameter just below stapes		.021
Distal diameters	long	.014
	short	.011
Diameters of head of epicolumella	long	.017
	short	.0095
Diameters of disk of stapes	long	.029
	short	.021

The shaft is slightly curved. The proximal extremity is divided by a fissure which is at right angles to the long transverse diameter. The smaller of these divisions is the more prominent, and its free extremital angle is formed by the continuous concave edge of the shaft. It bears the same relation to the shaft as the head of a rib does to its shaft (Fig. 1). The other proximal division occupies the position with reference to the shaft that the tubercle does to the rib. It is much larger than the inner head of the columella, and its face looks away from that of the head at an angle of 120°. Its long diameter diverges from that of the head by an angle of about 145°. Its free surface is a wide oval, and is concave, forming a basin-shaped lid to the foramen ovale of the internal ear. It thus represents the expanded proximal extremity of the stapes of other vertebrates. The base of this stapedial portion is perforated in the direction of its long diameter by a canal. One foramen of this canal is situated on the external edge below the external extremity of the oval basin. The other foramen issues in a groove, which continues for a short distance on the inner side of the bone from the fissure which separates the epicolumella from the stapes. This canal is, no doubt, that for the mandibular artery, and represents the foramen of the stapes, which is present in many Mammalia (Fig. 1 e e).

The distal extremity of the shaft is concave, and shows an articular surface of ridges and pits (Fig. 1 c). The coarseness of the latter indicates that the distal element attached at this point was cartilaginous, at least at the point of attachment. It will then resemble the corresponding part in the Crocodilia and Lacertilia, which connects the columella with the membranum tympani.

The points above determined as to the structure of this element permit of a number of interesting deductions.

First. This columella possesses what has not been previously observed in reptiles and higher

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* Proceedings American Philosophical Society, 1884, p. 30.

vertebrates, an osseous connection, distinct from that formed by the stapes with the foramen ovale of the os petrosum. From this it follows that the stapes cannot be regarded as the proximal extremity of the visceral arch of which the columella forms a part, as its appearance in other reptiles would lead us to infer. It also lends support to the view of Salensky, which is accepted by Fraser, that the stapes is not an ossification of the cartilage of the visceral arch, but is an ossification of the tissue surrounding the mandibular artery.

Second. That the stapes resembles that of the Mammalia, and differs from that of other reptiles in the perforation below its head.

Third. That it is succeeded distally by a cartilaginous element, as in many other reptiles, which is the triangular ligament of Cuvier, and is functionally the analogue, and probably the homologue of the malleus of the Mammalia.

The homology of the proximal extremity of this columella may now be considered. It cannot be the suprastapedial cartilage of Huxley, since that is a superior process of the distal cartilaginous element or malleus. It appears to be unrepresented in the reptilian columella, and I have therefore called it the *epicolumella** (Figs. 1, Ecol).

In order to obtain some light on the homologies of the parts of this element, I have compared it with the corresponding parts in various species of reptiles and batrachians, several of which have been figured by Messrs. Huxley, Peters, and Parker. I have examined the ear bones and cartilages of the *Heloderma suspectum*, and append herewith the result of my observations:

The columella has the length usual in the Lacertilia, ceasing a short distance proximad to the eustachian foramen. The cartilage, which continues in the same straight line, is divided at the eustachian foramen, one process passing downwards on its anterior border, the other forming its superior border. The posterior branch continues downwards for a short distance and terminates in a point, which is connected by a short ligament with the extremity of the pterygoid bone (Fig. 2 hl). Immediately exterior to it, a slender, rod-like ligament descends in close contact with it. It extends farther, however, reaching the articular bone of the lower jaw immediately posterior to the cotylus for the quadrate (Fig. 2 el). Its subsequent course will be mentioned below. It appears to be the ligament which Peters has represented as continuous with the descending process of the stapedial cartilage, and on which he based his belief in the continuity of the latter with the cartilage of Meckel. Its superior connection is, however, not with any part of the ossicula auditus, but it can be traced to a point above the external extremity of the exoccipital bone.

The stapedial cartilage extends beyond the superior edge of the large eustachian foramen to the membranum tympani, and is there decurved, extending in contact with it for 2–3 mm. and terminating in an acute apex. Near the point where it reaches the membrane it sends a branch upwards and backwards (Fig. 2 sst,) the suprastapedial cartilage, which forms a slender rod. The suprastapedial reaches inwards, and terminates at a point on the inferior side of the exoccipital bone at a point a little within opposite the origin of the inferior branch. It is only connected with the horizontal cartilage below it by membrane, and it does not form a fan-shaped plate as represented by Peters in Stellio and Huxley in Hatteria.

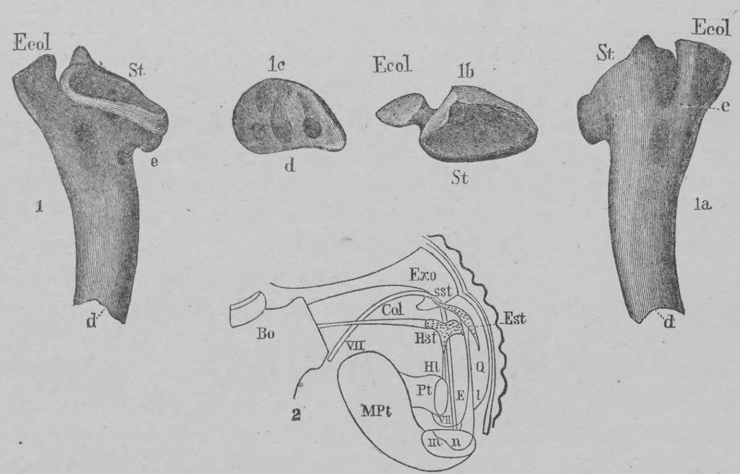
The following are the connections of the cartilages with adjacent elements: The distal extremity is acuminate and lies for a short distance on the membranum tympani, where it terminates without continuation. From the convexity of the curve formed by the inferior edge of the cartilage where it turns upwards, backwards, and inwards to form the suprastapedial, a narrow and weak band descends. It passes along the posterior border of the eustachian foramen, and terminates on the superior edge of the mandible. As it descends it thins out and becomes undistinguishable as a distinct rod or band. The slender rod already described as descending to the mandible from the descending process of the cartilage along the inner border of the eustachian foramen is figured by Peters in *Uromastix spinipes*.† He describes "it as a fibrous thread, which was formerly cartilaginous and connected the malleus with Meckel's cartilage." According to the figure it is not continuous with the inferior process of the cartilage ("malleus"). In *Heloderma suspectum* it passes anterior to the cartilage, in close contact with it, to a point superior to the suprastapedial process,

*American Naturalist, 1884, p. 1254.

†Monatsberichte Akademie Berlin, 1874, 44 f. B.

and then turns towards the base of the skull. I trace it directly to a foramen on the superior edge of the sphenoid. It is clearly the facial portion of the seventh nerve (tensor tympani), as described by Fischer and Stannius,* and has nothing to do with the auricular bones and cartilages. The only connection, then, with inferior arches which I can detect in this species is the fibrous one with the mandible, and I am doubtful of the significance of this.

It does not seem practicable to recognize the suprastapedial in the epicolumella of *Clepsydrops leptocephalus*.† It would require an excessive shortening of the columella, which might readily be the condition of things in Clepsydrops. But it would require that the suprastapedial should be ossified, and separated by suture from the remainder of the cartilage. Until some form is found in which this cartilage is segmented such a hypothesis has no foundation. The homology of the epicolumella with the incus is, on the other hand, almost certain; *first*, by the evident propriety of the exclusion of the stapes from the question, on account of its position, and by the history of its origin as shown by Salensky; *second*, on account of its position relative to both the stapes and the malleus. This being the case, the result follows that the doctrine of Peters that the quadrate bone is not the incus, as was maintained by Reichert, is the true one.‡



EXPLANATION OF PLATE.

FIG. 1. Columella auris of *Clepsydrops leptocephalus;* internal side. Fig. 1*a*, external side; 1*b*, proximal extremity; 1*c*, distal extremity; st., head of stapes; *Ecol.*, epicolumella; *d*, distal articular surface, especially represented in Fig. 1*c*; *e e*, foramina of stapedial canal. All figures are half natural size, excepting 1*c*, which is natural size.—From the proceedings of the American Philosophical Society, 1884, p. 46.

FIG. 2. Auricular bones and cartilages and adjacent parts of *Heloderma suspectum* Cope,§ twice natural size. *Bo.*, basioccipital bone; *Exo.*, exoccipital; *Q.*, quadrate; *Mn.*, mandible; *Pt.*, pterygoid; *M. Pt.*, internal pterygoid muscle; VII, seventh nerve; *Col.*, columella auris; *Hst.*, hypostapedial process of auricular cartilage; *Sst.*, suprastapedial process; *Est.*, epistapedial process; *Hl.*, hypostapedial ligament; *El.*, epistapedial ligament.

* Zoötomie der Fische, p. 154.

† Such a hypothesis is suggested after inspection of Huxley's figure of these parts in Hatteria, in Anatomy of Vertebrated Animals, p. 77, Fig. A. See also American Naturalist, 1884, p. 1253; Proceeds. Amer. Philosoph. Soc., 1884, p. 41.

‡ See Proceedings Amer. Philosoph. Society, 1884, p. 41, where Peter's view is maintained.

§ I owe the specimen dissected to my friend Horatio N. Rust, who obtained it on the Gila River, Arizona.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

EIGHTH MEMOIR.

ON THE STRUCTURE OF THE BRAIN OF THE SESSILE-EYED CRUSTACEA.

ON THE STRUCTURE OF THE BRAIN OF THE SESSILE-EYED CRUSTACEA.

READ AT WASHINGTON, APRIL 14, 1884.

By A. S. Packard.

The following descriptions and notes have grown out of an attempt to compare the nervous system, particularly the brain and other ganglia of the head, of the eyeless species of cave-inhabiting Arthropods with their out-of-door allies. We have begun with the structure and morphology of the brain of *Asellus communis* Say as a standard of comparison with that of the blind Asellid, *Cecidotæa stygia* Pack., which is so common in the brooks of Mammoth and other caves and in the wells of Southern Indiana and Illinois. Studies of this nature are, it seems to us, well calculated to throw light on the origin of the cave forms, and to show what great modifications have been produced in these organisms by a radical change in their surroundings; consisting, as it does, mainly in the absence of light, and perhaps of the usual food, or at least the usual amount of food.

It is plain enough that the species of Cecidotæa are simply eyeless, slender, depauperated Aselli, which have originated from some one of our out-of-door species within a comparatively recent time, at least since the river-terrace epoch of the Quaternary Period. The facts bearing upon the general relations of the blind to the eyed Asellidæ, and a discussion of the change in form of the body and its appendages, and of the causes of the transformation of the species and genus, are reserved for another occasion.

My present purpose is simply to describe and depict the brain and other nerve-centers of the head of *Asellus communis* Say and *Cecidotæa stygia* Pack.

I. The brain of Asellus communis.

The nervous system of the European *Asellus aquaticus* Linn. has been referred to by Leydig and also by Sars, who published a figure of the nervous system as a whole. Leydig's "*Vom Bau des thierischen Körpers*" gives a careful and comprehensive general account of the nervous system of Arthropods, the most complete and authoritative, up to 1864, we possess, supplemented as it is by his excellent *Tafeln von vergleichenden Anatomie*, published in the same year (1864). According to Leydig, in the Isopoda (Oniscus, Porcellio) the optic lobes are very large and overlie the cerebral lobes.

In *Asellus aquaticus* the abundant fat body around the ventral cord belongs to the blood sinus which envelops the nervous cord. Of this form Leydig has little to say, remarking that he did not examine the entire ventral cord, but only sections, which agree in appearance with those of the land wood-lice.

Sar's figure of the brain of *Asellus aquaticus* is drawn on a small scale, is rather indifferent, and does not show more than the cerebral lobes and optic nerves. He evidently did not perceive the other ganglia.

Leydig's valuable figures of the brain of *Oniscus murarius* show that he did not study the nervous centers of the head by means of longitudinal sections, and that he simply dissected the brain from above, a dorsal view showing the large optic lobes to be mostly above and in front of the smaller cerebral lobes, while the ganglion, *e*, in his figure 8 (Taf. VI), which he denominates *nebenlappen*, is probably one of the antennal ganglia. The other ganglia of the head he does not represent, nor speak of in his Vergleichende Anatomie.

The other sketches of Isopod brains by Brandt and Ratzeburg, Rathke, Lereboullet, and Milne-Edwards, as well as those in our "Zoology,"* are drawn on a small scale, are in some cases rather indifferently drawn, and only represent a dorsal view, the antennal and those ganglia posterior to it being concealed from view in dissecting from above downward.†

The observations I have made are based on vertical, longitudinal sections kindly made for me by Mrs. C. O. Whitman, under the direction of Dr. C. O. Whitman. The sections were thin, clear, well-mounted in Canada balsam, in consecutive order, and made from alcoholic specimens, which had, however, been kept for several years, though the nervous system had been well preserved.

THE HISTOLOGICAL ELEMENTS OF THE GANGLIA.

Unlike the central nervous system of Vertebrates, in which there are but two kinds of nerve tissue, viz, ganglion cells and fibers, there are in the Asellidæ, as in insects and Decapods, three kinds of elements in the brain and other ganglia, viz: (1) ganglion cells; (2) nerve fibers; and (3) Leydig's *puntzsubstanz* (*marksubstanz* of Leydig and Rabl-Rückhard, and especially Dietl), which might be called the *myeloid* tissue or substance.

(1) *Ganglion cells.*—These have not, as in the brain of the lobster, a simple nucleus and nucleolus, but they usually have numerous, from 10 to 20, nuclei, the nucleolus of each nucleus readily receiving a stain and forming a distinct dark mass. They resemble those of the locust.‡ They are, as a rule, much smaller, however, than in the locust. As seen in most of the sections they appear to be spherical, being cut through transversely by the microtome, but as shown by Fig. 3*a* they are of the usual pyriform shape. In size they are very much smaller than those of the lobster and much more uniform in size, very few of the cells being twice as large as those of the average size; as already remarked, the nucleus in the ganglion cells of the American lobster are almost uniformly simple and homogeneous, with a single nucleolus. The largest ganglion cell of the lobster's brain which we have found is six times as large as the largest ganglion cell of Asellus.

The ganglion cells appear to be entirely unipolar; no bipolar or multipolar cells were observed, though special search was made for them. Nothing noticeable was observed in respect to the nerve-fibers. The *punktsubstanz*, *marksubstanz* or myeloid substance, as we may designate it, differs in its topographical relations from that of the brain of Decapoda. This myeloid substance, which seems to be peculiar to the worms, mollusks, and especially the crustacea and insects, has been most thoroughly studied by Leydig. This is the central finely-granular part of the brain, in which granules have short irregular fibers passing through them. In his *Vom Bau des thierischen Körpers*, p. 89, Leydig thus refers to it:

> In the brain and ventral ganglia of the leech, of insects, and in the brain of the Gastropods (Schnecken) I observe that the stalks (stiele) of the ganglion-cells in nowise immediately arise as nerve-fibers, but are planted in a molecular mass or *punktsubstanz* situated in the center of the ganglion, and merged with this substance. It follows, from what I have seen, that there is no doubt *that the origin of the nerve-fibers first takes place from this central punktsubstanz.*
>
> This relation is the rule. But there also occur in the nerve-centers of the invertebrates single definitely situated ganglion cells, whose continuations become nerve-fibers without the intervention of a superadded punktsubstanz.

Leydig subsequently (p. 91) further describes this myeloid substance, stating that the granules composing it form a reticulated mass of fibrillæ, or, in other words, a tangled web of very fine fibers.

> We at present consider that by the passage of the continuation of the ganglion cells into the *punktsubstanz* this continuation becomes lost in the fine threads, and on the other side of the *punktsubstanz* the similar fibrillar substance forms the origin of the axis-cylinders arranged parallel to one another; so it is as good as certain *that the single axis-cylinder derives its fibrillar substance as a mixture from the most diverse ganglion cells.*

The myeloid substance in the brain of Asellus is not however differentiated into distinct spherical masses, the *punktsubstanzballen* of Krieger (*Balken* of Dietl) or whitish ball-like masses

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* Fig. 255, *Idotœa inorata*, and Fig. 256, Serolis, drawn by J. S. Kingsley.

† Since this essay has been prepared I have obtained Dr. Bellonci's excellent memoir on the nervous system of Sphæroma, in which he figures and describes the brain and nervous system in general of that Isopod.

‡ Second Report United States Entomological Commission, ch. xi. The Brain of the Locust, 1880 (Pl. xi, Fig. 3b–3e).

which are so characteristic of the brain of the Decapod Crustacea and the insects; and in this respect there is probably a wide difference between the brain of Decapoda and Edriophthalmata.

HISTOLOGICAL TOPOGRAPHY OF THE NERVE-TISSUES.

(1) *The ganglion cells.*—These cells form a cortical layer enveloping on all or nearly all sides the central myeloid mass. The cells being distinct and more or less loosely arranged readily take a deep carmine stain, while the much more dense myeloid mass remains white and unstained.

The ganglion cells are collected into more or less definite masses, enveloped by connective tissue, the latter as it were forming a mesh, inclosing spherical masses of ganglion cells. In a vertical section, such as that represented by Figs. 2 and 3, passing through the anterior and middle part of the brain and in the horizontal section (Fig. —), while the ganglion cells are seen to be packed more or less solidly around the central myeloid portion, they are also seen to be disposed in more or less distinct lobular masses, which are inclosed by connective tissue. Seven or more distinct lobes or subspherical masses of these ganglion cells may be distinguished on each side of the brain.

As seen in Figs. 2 and 3, the uppermost or dorso-frontal lobes are the double sets filling the upper or dorsal fissure between the right and left lobes of the brain and marked *a* and *b*; *b* is divided into two sublobes, the upper (b^1) being small, flattened, and lying on the dorsal and inner edge of the central lobe. The third set is a double lobe, $c\ c^1$; these may be called the dorso-lateral set; they are more or less connected with the lateral lobes $d\ d^1$, and the latter with the externo-commissural set of lobes ($e\ e^1$). On the dorsal side of the brain near the base of the optic ganglia are two sets, one above and one below (*g*) the base of the optic ganglion; the exact relation of these to the others is not very plain from our sections, but they are in front of and external to the outer edge of the lobes of the brain.

The optic ganglion is enveloped by a lobulated mass of ganglion cells exactly like those of the brain proper, and these lobes (*h i k*, Fig. 27) which envelop the myeloid mass can be distinguished from the outer one at the beginning of the outer division of the nerve fibers sent to the eye from the ganglion cells.

(2) *The nerve fibers.*—The fibers arising from the ganglion cells form the commissures which unite the brain with the subœsophageal and succeeding ganglia; and also the commissures between the two cerebral lobes.

One set of fibres arise in the dorso-frontal group of ganglion cells (Fig. 3, *f b*), to become lost in the myeloid substance. The fibers are seen to pass down, and to form a part of the subœsophageal commissure, although we did not trace them to the last abdominal ganglion. Judging from Michel's observations on the commissural fibers of *Oryctes nasicornis*,* there is little doubt but that in all Arthropoda certain nerve-fibers arising in the pro cerebral lobes pass uninterruptedly to the last ventral ganglion.

It will be further seen by reference to Figs. 2, 3 (Asellus), and especially Fig. 27 (Cecidotæa), that the fibers arising from certain of the ganglion cells in lobes *c* and c^1 pass into the cerebral lobe in two directions, some connecting the two lobes, forming the transverse commissure, while others pass down and run parallel with the fibers from the dorso-frontal lobes and aid in building up the subœsophageal commissures. The latter commissure is also re-enforced by fibers from the lateral lobes $d\ d^1$, $e\ e^1$.

From what we have seen in the sections represented by the camera sketches referred to (Figs. 2, 3, and 27), and from what is known of the cells and fibers of other Arthropods, there is no doubt but that all the ganglion cells give rise to fibers, some of which at least pass directly through or above or around the myeloid substance of the cerebral lobes and form the commissures. This independence of the myeloid substance appears to be more general in the Asellidæ, at least this we would infer from Leydig's statements previously quoted. When we look at Fig. 1, which is a composition (drawn, however, with the camera) from the sections represented by Figs. 5 and 8 we see that the two main longitudinal commissures pass above the seven post-cephalic ganglia represented in the figure. Those ganglia are masses of myeloid substance, with a cortical layer of gan-

* Michels. Beschreibung des Nervensystems von *Oryctes nasicornis* in Larven, Puppens und Käferzustand. Zeits. f. wissens. Zoologie., xxxiv, 641–702. 1880.

glion cells, from which fibers arise after passing through the myeloid substance; there becoming broken up into a tangled mass of fibrillæ, which unite finally to form the fibers constituting the nerves of the appendages. Without doubt also a few commissural fibers from the procerebral lobes pass into each post-cerebral ganglion so as to afford the means to the cerebral lobes (*primi inter pares*, as happily styled by Leydig) of coördinating the nervous power of the other ganglia, their histological and morphological equivalents. It should be said that although Leydig's view as to the relations of the nerve-fibers to the myeloid substance may be the correct one, yet though it may apply to the Annelids, it may not be so general an occurrence in the Arthropods. It seems to us, though we are still open to conviction, that the transverse and longitudinal commissural fibers, which undoubtedly arise from the cortical ganglion cells, have little or nothing to do with the myeloid substance. This latter substance does not exist in the nervous system of the vertebrates, and just what its nature and function clearly are in the invertebrates has yet to be worked out. In the hands of a skillful and expert histologist, much light will yet be thrown upon this difficult subject; certainly the present writer has not the qualifications for the task. His own opinion from what little he has seen is, that the myeloid substance is the result of the splitting up into a tangled mass of very fine fibrillæ of certain of the fibers thrown off from the mono-polar ganglion cells, *i.e.*, such fibers as do not go to form the main longitudinal commissures. It should also be borne in mind that in the embryo the ganglia are composed of ganglion cells alone, with few if any primitive fibers.

MORPHOLOGY OF THE BRAIN.

The brain of the Isopods and Amphipods is a *syncerebrum*, though far less complicated than in the Decapoda. It will be remembered that Professor Lankester in his memoir on Apus designates the simple brain of that crustacean as an *archicerebrum*, while the composite brain of "all crustacea, excepting Apus, and possibly some other Phyllopods," he denominates a *syncerebrum*. In our Monograph of N. A. Phyllopoda, p .403, we adopted the view that the brains of all Crustacea except the Phyllopoda and Merostomata were syncerebra, and we divided the syncerebrum into three types; adding that the syncerebrum of sessile eyed crustacea (Edriophthalma) was built on a different plan from that of the Decapoda.

Fig. 1 has been drawn to give a general view of the nervous centers of the head, including the first thoracic segment and its ganglion. It has been drawn with the camera from a number of sections, especially those represented by Figs. 5–8, so that it is believed to be approximately correct and not merely a schematic plan. The section passes through the head on one side of the œsophagus, which of course is not represented in the sketch; being so near the median line it does not involve the optic lobes and eyes, which, especially the latter, are on the extreme side of the body, so that these organs could not well be shown in the drawing. The general relation of the nervous system to the body walls, to the stomach and the appendages are made obvious in the sketch, and their description need not detain us. It should be borne in mind that the mouth and œsophagus open between the mandibles. They are shown in Fig. 5. The end of one of the ovarian tubes is seen to overlie the pyloric end of the stomach; it does not pass into the head. The drawing of the heart is somewhat diagrammatic, as it was not well shown in the sections, but its position is believed to be approximately correct. The sympathetic nerve was not discovered.

As seen in Fig. 1, the brain or supraœsophageal ganglion is a composite mass or group of four pairs of ganglia, *i. e.*, (1) the brain proper or procerebral lobes, (2) the optic ganglia, (3) the first antennal, and (4) the second antennal lobes. These lobes are quite separate from each other in the Isopoda and Amphipoda as compared with the Decapoda.

THE PROCEREBRUM OR PROCEREBRAL LOBES.

These constitute the brain proper, and have been usually called the "cerebrum" or "cerebral lobes." As, however, they are not the homologues of the lobes of that name in Vertebrates, either structurally or functionally, we would suggest that the ganglion be termed the *procerebrum* and the individual lobes the *procerebral lobes*, not only in allusion to its position in advance of all the other ganglia, but since it stands as the co-ordinating, regulating ganglion, the first in importance of all the ganglia.

As regards size, the procerebral lobes are more than double that of the other ganglia; they bulge out dorsally and backward, so as to conceal from above the antennal and mandibular ganglia. Plate 1, Fig. 2, represents a section through the lobes in front of the commissure, showing at a, b, the dorso-frontal group of ganglion cells, those nearest the myeloid substance sending fibers downward (*fb*) to form a part of the œsophageal commissure. At Fig. 3, a section farther back and passing through the commissure, the fibers are seen to pass directly through the myeloid substance along the inner side of the commissure. Fig. 4 represents a still more posterior section; this shows distinctly the origin of the fibers of the transverse commissure (*tr. c*) from the ganglion cells of the upper and outermost portion of the lobes. The commissure is seen to be composed of three bundles of fibers—an upper, middle, and lower or ventral; the space between the upper and middle bundles being filled with myeloid substance.

Vertical sections of the procerebral lobes are seen in Figs. 5 to 8. Fig. 5, which passes through the median line of the head, through the mouth, œsophagus, and the median line of the stomach, shows the procerebral lobe on one side of the commissure; and, below, the second maxillary and maxillipedal ganglia. Fig. 7, passing through one side of the first antennal ganglion, shows the procerebral lobe nearly separate from the antennal lobe. Fig. 8 represents a section passing through the main commissure and a portion of the procerebral lobe.

Horizontal sections from the top of the head downwards are seen in Figs. 9 to 18. Fig. 9 represents a section through the upper part of the procerebral lobes; Fig. 10, through the lobes above the transverse commissure; Fig. 11, through the entire procerebrum, near the origin of the optic ganglia and optic nerves.

THE OPTIC GANGLIA AND OPTIC NERVES.

The eyes being smaller in Asellus than in most other genera of Isopods, particularly Oniscus and Porcellio, the forms figured and described by Leydig; the optic ganglion and nerve are also much smaller, while the eyes being set farther back on the sides of the head, the ganglion and nerve are directed obliquely backward, so that a series of vertico-frontal sections pass through the brain before reaching the optic nerve. Pl. IV, Figs. 19–21, represent these organs. Fig. 19 shows the procerebral lobes, and on the left the optic ganglion and the optic nerve leading to the eye. Fig. 20 represents a section just behind the procerebral lobes, passing through the hinder edge of the cortical layer of ganglion cells. Fig. 21 is an enlarged view of the sam e. The optic lobe is divided into two parts, the inner connected with the procerebral lobe, with an abundant supply of ganglion cells, while from the smaller, outer division arise the fibers which unite to form the optic nerve, which divides at or just beyond the middle into several branches sent to the eyes. These branches are seen to end in slightly bulbous expansions among the small retina cells, forming the deep brown pigment-mass in which the lenses are imbedded.

The first antennal ganglia (Figs. 1, 7, and 12).—The relations from a side view to the other parts of the brain are seen in Figs. 1, 7, and 7*a*. It will be seen that the ganglion is much freer from the procerebral lobes than in the Decapoda. It may be seen from above, when looking down upon the brain, projecting somewhat in advance of the procerebral lobes, the first antennal nerve arising from the upper and anterior side, ascending a little at its origin, and passing horizontally into the base of the antenna. Fig. 12 represents a horizontal section through the lobes, showing the ganglion cells, the myeloid substance, and the origin of the antennal nerves.

The second antennal lobes (Figs. 1, 7, 7*a*, 14 to 16).—The second antennal ganglion lies directly beneath the upper or first antennal lobes, and appears to be slightly larger than the latter, the nerves being larger, corresponding to the much larger size of the second antenna. It will be seen by reference to Figs. 14 to 16 that the œsophagus passes between the lower part of the lobes, which are almost wholly separate. (Figs. 17 and 18, which represent sections just below that represented by Fig. 16, are introduced to show the œsophageal commissures and their ganglion cells on each side of the œsophagus.)

The first subœsophageal or mandibular ganglion (Figs. 1, 6, 7, 22, 23, *md. g.*).—This is rather larger than either of the antennal ganglia, as its relations to the brain are well seen in the sections represented by Fig. 6. By reference to the sections represented by Figs. 5 and 6, it is clearly seen to lie directly under the antennal ganglia, and to be separated from the brain proper by the short œsophageal commissures. It is therefore the first subœsophageal ganglion, givin g off but a single pair of nerves, those supplying the large tripartite mandibles.

The ganglion lies in front of the main longitudinal commissure, and in position in front of the lower side of the stomach, being situated in an inclined plane, nearer vertical than horizontal The sections represented by Figs. 22 and 23 pass through a portion of it, and in them is well seen. the mode of origin of the large mandibular nerves.

The first and second maxillary ganglia.—These are situated widely apart, neither coalescing with the other ganglia in front or behind. The first maxillary ganglion (Figs. 1, 8, 22, 23, *mx. g.*) is situated nearer the mandibular than the second maxillary ganglion, as seen in Figs. 1, 22, and 23. It lies in an inclined plane, and is much smaller than any of the other postœsophageal ganglia, as it innervates smaller appendages.

The second maxillary ganglion (Figs. 1, 8, 22, 23, *mx² g.*) is situated next to the maxillipedal ganglion, and like that lies in a horizontal position. It is of nearly the same size but a little smaller than the ganglion next behind it, and the commissures connecting it with the maxillipedal ganglion are very short.

The maxillipedal ganglion (Figs. 1, 8, 22, 23 *mx p. g.*) is a little larger than its near neighbor, the second maxillary ganglion, inasmuch as it innervates the large maxillipedes.

At some distance behind this ganglion and situated in the first thoracic segment is the first thoracic ganglion supplying the nerves to the first pair of feet. It is a little larger than the maxillipedal ganglion.

The main longitudinal commissures (Figs. 1, 22, 23) pass over the ganglia, and are united in the head, except at two points indicated by the clear spaces in the figure, behind which point we have not traced it. Sars, however, represents the main longitudinal commissure behind the head as double.

In the section represented by Figs. 22 and 23 the limits of the mandibular and first maxillary ganglia are not definite, and they are seen to be connected by a bridge or tract of myeloid substance. Towards the second maxillary ganglion the fibers in the section are fewer and lower together, and are seen in some cases to enter the myeloid substance, but in others to pass over it. The ganglion cells of the maxillipedal ganglion are more numerous than those about the myeloid mass of the second maxillary ganglion

From the foregoing facts it will be seen that the brain of the *Asellidæ* is composed of four preœsophageal pairs of ganglia, situated at greater or less distance apart from each other, being a very loosely constructed syncerebrum compared with that of such Decapods as have been thus far examined. The mouth-parts in the *Asellidæ*, if not all Isopoda, are not innervated from a single subœsophageal ganglion, but each appendage, beginning with the mandibles, is supplied by a nerve arising from a separate ganglion. Thus there are eight ganglia of the first order in the head of these Isopods, our observations not referring to any secondary ganglia, which may or may not exist in connection with the brain or sympathetic nervous system. It will be remembered that in the Decapods, the lobster for example, the brain innervates the eyes and antennæ, while the only other ganglion in the head is the subœsophageal, from which the mouth appendages are all innervated; thus there are but two nerve-centers in the head of adult Decapods; the subœsophageal ganglia being concentrated probably during embryonic or larval life.

II. The brain of the eyeless form Cæcidotæa.

It is a matter of great interest to know just what, if any, changes take place in the brain or nerve-centers of the head of the eyeless forms related to Asellus; whether the modification is confined to the external parts of the eye, or to the optic lobes and nerves alone.

It is well known that a blind Asellus-like form is abundant in the brooks and pools of Mammoth and other caves in Kentucky and Indiana, as well as in the wells of the cavernous and adjacent regions. The foregoing observations on the brain and eyes of the common Asellus of our brooks and ponds were made to afford a basis of comparison with the similar parts in the eyeless form.

Cæcidotæa in its external shape is seen to be a depauperate Asellus, with the body, however, much longer and slenderer than in the eyed form, and with slenderer appendages. It is not usually totally eyeless. In a number of specimens from a well at Normal, Ill., kindly sent us by Mr. S. A. Forbes, a minute black speck is seen on each side of the head in the positions of the eyes of Asellus, just above the posterior end of the base of the mandibles. In some specimens these black dots are not to be seen; in others they are visible, but fainter than in others. In twelve specimens which I collected in Shaler's Brook in Mammoth Cave I could detect no traces of eyes, and infer that most, if not all, the Mammoth Cave specimens are totally eyeless. It thus appears that different individuals have eyes either quite obsolete, if living in caves in total darkness, or, if living in wells, with eyes in different degrees of development up to a certain stage—that represented by black dots—which, however, are so easily overlooked, that we confess, after handling dozens of specimens, we did not suspect that the rudimentary eyes existed, until our attention was called to them by Dr. C. O. Whitman when he sent the slides. The European *Cæcidotæa forelii* is also said to be blind. The specimens we received through the kindness of Professor Forel, which were, unfortunately, dried and spoiled, seemed to be entirely eyeless, though special search was not made for the eye-specks.

It will be seen that the eyeless Cæcidotæa differs from Asellus as regards its brain and organs of sight, in the complete loss of the optic ganglion, the optic nerve, and the almost and sometimes quite total loss of the pigment-cells and lenses.

After a pretty careful study of numerous vertical sections of the brain of *Cæcidotæa stygia* as compared with that of *Asellus communis* we do not see that there are any essential differences, except in the absence of the optic ganglia and nerves. The proportions of the procerebral lobes, of the ganglion cells, their number and distribution, the size of the transverse and longitudinal commissures are the same. The head and brain as represented is smaller than in Asellus, the form itself being considerably smaller.

Fig. 25 represents a section through the middle of the procerebral lobes, which may be compared with that of Asellus, Fig. 4. Another section a little posterior is represented by Fig. 26. Fig. 27 is an enlarged view of a section still further back, which shows that there is little, if any, difference between the brain at this point and that of Asellus represented by Fig. 3. In this section it is easy to see that the ganglion cells on each side of the procerebral lobes send fibers directly through the myeloid mass to form the transverse commissures. The section at this point does not show the fibers arising from the fronto-dorsal group of ganglion cells; but traces of them are seen in Fig. 28, which represents a section corresponding to that indicated by Fig. 3.

Careful examination of the sections passing behind the procerebral lobes and œsophageal commissures failed to show any traces of the optic ganglion of either division, or of the ganglion cells and myeloid substance composing it. Every part connected with the optic ganglia seems to be totally abolished. The same may be said of the optic nerve throughout its length. The amount of time spent in examining the numerous well cut, thin, and beautifully mounted sections made by Dr. Whitman, or under his direction, enables us to affirm positively that the entire nervous portion of the optical organs are wanting. And we are glad to add that Dr. Whitman also observed to us the absence of the optic nerves.

With the eye itself it is different. The modification resulting from a life in total darkness has left traces of the eye, telling the story of degeneration and loss of the organs of sight, until but the merest rudiments of the eye remain as land marks pointing to the downward path in degradation and ruin taken by the organs of vision as the result of a transfer to a life in total or partial darkness, as the case may have been, in the well-inhabiting or cave-dwelling individuals.

Fig. 29 represents a section through the head of *Cæcidotæa stygia* behind the procerebral lobes and œsophageal commissures, showing the absence of any traces of the optic ganglia or optic nerves, but indicating the rudiments of the eye, showing that the pigment mass of the retina and the lenses exist in a very rudimentary condition, while the optic nerve and ganglion are entirely aborted.

Figs. 30 and 31 represent enlarged views of the rudimentary eye of two different specimens of *C. stygia* from Mammoth Cave. In the sections represented by Fig 30 *a b* we see that the number of facets has been reduced apparently to two (*b*), the rudimentary lenses being enveloped by a black pigment mass. This section, examined by Tolles' $\frac{1}{5}$ A, is magnified and drawn to exactly the same scale as that of the eye of Asellus represented by Fig. 21. In that figure may be seen the normal size of the lenses and of the retina cells. It will be seen that in Cæcidotæa the retina cells are broken down and have disappeared as such, and that the rudimentary lens (or the hyaline portion we suppose to be such) which the retinal pigment incloses is many times smaller than in the normal eye of Asellus.

On comparing the eyes of the two specimens as shown in Figs. 31*a* and 32*a*, it will be seen that the eyes in one are considerably larger than in the other specimen. Fig. 32*b* shows that in the eye of this individual there were at least four lenses, if not more, not included in the section. At the point indicated by 32*d* on the edge of the eye one lens is indicated (though the divisions are wanting), not wholly concealed by the pigment of the retina; a more magnified view is seen at Fig. 32*e*. The four sections *a–d* passed through the eye, the section in front and behind not touching the eye itself.

It thus appears from the observations here presented that the syncerebrum of the blind Cæcidotæa differs from that of the normal Asellus in the absence of the optic ganglia (both divisions) and the optic nerves, while the eyes are exceedingly rudimentary, the retinal cells being wanting; the black pigment mass inclosing very rudimentary minute lens-cells, which have lost their transverse zonular constriction or division; the entire eye of Cæcidotæa finally being sometimes wanting, but usually microscopic in size, and about one-fifth as large as that of the normal Asellus.

The steps taken in the degeneration or degradation of the eye, the result of the life in darkness, seems to be these: (1) the total and nearly or quite simultaneous loss by disuse of the optic ganglia and nerves; (2) the breaking down of the retinal cells; (3) the last step being, as seen in the totally eyeless form, the loss of the lens and pigment.

That these modifications in the eye of the Cæcidotæa are the result of disuse from the absence of light seems well proved; and this, with many parallel facts in the structure of other cave Crustacea, as well as insects, arachnids, and worms, seems to us to be due to the action of two factors: (a) change in the environment; (b) heredity. Thus we are led by a study of these instances, in a sphere where there is little, if any, occasion for struggling for existence between these organisms, to a modified modern form of Lamarckianism to account for the origination of these forms, rather than to the theory of natural selection, or pure Darwinism as such.

BIBLIOGRAPHY OF WORKS ON THE NERVOUS SYSTEM OF CRUSTACEA.

GENERAL.

BELLONCI, G. Intorno alla struttura e alle connessioni dei lobi olfattorii negli Artropodi superiori e nei vertebrati. 2 tav. 4. Ivi, 1882.

BERGER. Untersuchungen über den Bau des Gehirns und der Retina der Arthropoden. Arbeiten des zool. Instituts zu Wien, Heft 2.

——— Nachtrag zu den Untersuchungen über den Bau des Gehirns und der Retina der Arthropoden. Ibid., Heft 3.

EHRENBERG. Beobachtung einer bisher unerkannten Structur des Seelenorgans. 1836.

GRENACHER, H. Untersuchungen über das Seeorgan der Arthropoden, insbesondere der Spinnen, Insecten, und Crustaceen. Göttingen, 1879.

HANNOVER. Recherches microscopiques sur le système nerveux. 1857.

HELMHOLTZ. De fabrica systematis nervosi evertebratorum. Diss. Berolini, 1842.

LEYDIG. Lehrbuch der Histologie des Menschen und der Thiere. 1857.

——— Vom Bau des thierischen Körpers. Erster Band. 1864.

——— Tafeln zur vergleichenden Anatomie. Erstes Heft. 1864.

MILNE-EDWARDS, H. Histoire naturelle des Crustacés. Tom. i–iii. Paris, 1834–1840.

REMAK. Ueber d. Inhalt der Nervenprimitivröhren. Archiv f. Anat. u. Phys., 1843.

WALTER. Mikroscopische Studien über das Centralnervensystem wirbelloser Thiere. 1863.

CIRRIPEDIA.

BRANDT, E. Ueber das Nervensystem der Lepas anatifera (anatomisch-histologische Untersuchung). Mélanges biologiques, vii. Bull. de l'Acad. imp., xv. St. Petersburg. June, 1870.

CUVIER, G. Mémoire sur les Mollusques. Paris, 1817.

CLAUS, C. Die Cypris-ähnliche Larve (Puppe) der Cirripedien und ihre Verwandlung in das festitzende Thier. Schriften der Gesellschaft z. ges. Naturw. Marburg. Supp.-Heft v. Marburg u. Leipzig, 1869. (Brain of larva of Conchoderma.)

DARWIN, C. A monograph of the sub-class Cirripedia. 1851, 1853.

KINGSLEY, J. S. Barnacles. American Naturalist, xi, 102. 1877, and Packard's Zoology, Fig. 228. (Nervous system of Lepas fascicularis.)

MARTIN ST.-ANGE. Mémoire sur l'organisation des Cirripèdes. Paris, 1835.

WYMAN, J. Proceedings Boston Soc. Nat. Hist. Amer. Jour. Sci. Arts, xxxix, 182. 1840. (Nervous system of *Otion cuvierii.*)

ENTOMOSTRACA.

CLAUS, C. Beiträge zur Kenntniss der Entomostraken. Marburg, 1860. (Nervous system of *Saphirina fulgens.*)

——— Die freilebenden Copepoden. Leipzig, 1863. (Nervous system of Copilia, Corycæidæ, Saphirella, Calanella, Cetochilus, Saphirina, Dias longiremis.)

——— Ueber die Entwickelung, Organization und systematische Stellung der Arguliden. Zeits. wissens. Zool., xxv, 1875. (Nervous system of Argulus foliaceus and coregoni.)

GEGENBAUR. Mittheil. üb. d. Organization von Phyllosoma und Sapphirina. Archiv f. Naturgeschichte, Taf. v, fig. 1, 1858.

NORDMANN. Mikrographische Beiträge. Heft 2, 1832.

ZENKER. Anat. syst. Studien über die Krebsthiere. Archiv f. Naturg., Taf. vi, fig. 13, 1854.

BRANCHIOPODA.

BRONGNIART, A. Mémoire sur la Limnadia. Mém. du Muséum, vi, 1820.

CLAUS, C. Zur Kenntniss des Baues und der Entwicklung von Branchipus stagnalis und Apus cancriformis. Abh. k. Gesell. Wissens., Göttingen, Bd. viii, 1873. (Brain of young B. stagnalis and A. cancriformis.)

——— Zur Kenntniss der Organization und des feineren Baues der Daphniden und verwandter Cladoceren. Zeits. wissens. Zool., xxvii, 1876. (Brain of Daphnia.)

——— Untersuchungen zur Erforschung der genealogischen Grundlage des Crustaceen-Systems. Wien, 1876. (Nervous system of young Estheria ticinensis.)

GRUBE, A. E. Bemerkungen über die Phyllopoden. Archiv f. Naturg., xix. Berlin. 1853. Nervous system of Limnetis.

JOLY. Recherches zool., anat. et phys. sur l' Jsaura cycladoides. Annales d. Sc. Nat., 1842.

LANKESTER, E. R. Observations and reflections on the appendages and on the nervous system of Apus cancriformis. Quart. Jour. Micr. Sc., April, 1881.

LEYDIG, FR. Naturgeschichte der Daphniden. Tübingen 1860. (Brain of several species of Daphnia, Sida, Lynceus, Pasithea, Bosmina, Polyphemus, and Bythotrephes.)

——— Verg. Anatomie. 248, 1864.

——— Ueber Artemia salina u. Branchipus stagnalis. Beitr. z. anat. Kenntniss dieser Thiere. Zeits. f. wissen. Zoologie, 1851.

MÜLLER, P. E. Danmarks Cladocera. Kroyer's Tidsskrift, Ser. iii, Bind v, 1868–9. (Brain of Bythotrephes, Podon, Evadne, Leptodera.)

PACKARD, A. S., jr. Monograph of the Phyllopod Crustacea of North America, with remarks on the order Phyllocarida. Twelfth Annual Rep. U. S. Geol. Surv. Terr., 1883. (Nerv. system of Apus lucasanus. Pl. xxxii, fig. 1. In this figure the lower loop between the liver and œsophagus should be removed and the commissure extended from above to take its place. Nervous system of Estheria mexicana and Branchipus vernalis. Plate xxxiii.)

SPANGENBERG, FR. Bemerkungen zur Anatomie der Limnadia Hermanni Brongn. Zeits. wissens. Zool., xxx, Supp., 474, 1878. (Nervous system described at length.)

WEISMANN, A. Ueber Bau und Lebenserscheinungen von Leptodora hyalina Lilljeborg. Zeits. wissens. Zool., xxiv, 1874.

ZADDACH, E. G. De Apodis cancriformis. Anatome et Historia evolutionis. Bonnæ, 1841. (Nervous system of Apus cancriformis.)

PHYLLOCARIDA.

CLAUS, C. Untersuchungen zur Erforschung der genealogischen Grundlage des Crustaceen-Systems. Wien, 1876. (Nervous system of Nebalia geoffroyi.)

PACKARD, A. S., jr. Monograph of the Phyllopod Crustacea of North America, with remarks on the order Phyllocarida. Twelfth Ann. Rep. U. S. Geol. Surv. Terr., 1883. (Brain and nervous system of Nebalia bipes.)

EDRIOPHTHALMA.

AUDOUIN ET MILNE-EDWARDS. Annales des Sc. Nat., xiv, 1828. (Nervous system of Talitrus and Cymothoa.)

BELLONCI, G. Sistemo nervoso e organi dei sensi della. Sphæroma serratum. Constar, Roma, 1881.

BRANDT UND RATZBURG. Medicinische Zoologie, 1829. (Nervous system of Oniscus.)

BRUSELIUS. Beitrag zur Kenntniss vom inneren Bau der Amphipoden. Archiv f. Naturgesch., 1859.

CLAUS, C. Untersuchungen zur Erforsch. d. Genealog. Grundlage des Crust. Syst. Wien, 1876. (Brain and organ of hearing of Oxycephalus figured.)

DE LA VALLETTE, G. De Gammaro puteano. 1857.

DOHRN, A. Naturgeschichte der Caprella. Zeits. wissens. Zool., xvi, 245, 1863. (Includes that of Cyamus ceti.)

——— Untersuchungen über Bau und Entwicklung der Arthropoden, 4. Entwicklung und Organization un Praniza (Anceus) maxillaris. 5. Zur Kenntniss des Baues von Paranthura costana. Zeits. wissens. Zoologie, xx, 1870.

FREY UND LEUCKART. Lehrbuch der Zootomie. 195, 1847. (Nervous system of Idothea and Caprella.)

GAMROTH, A. Beitrage zur Kenntniss der Naturgeschichte der Caprellen. Zeits. wissens. Zoologie, xxxi. (Brain of Caprella æquilibra.)

GERSTAECKER, A. Bronn's Classen und Ordnungen der Thierreichs. Arthropoda. Abth., ii, 1880.

KINGSLEY, J. S. Figures of the nervous system of Idotæa irrorata and Serolis guadichaudi. In Zoology for high schools and colleges, by A. S. Packard, Jr. New York, 1878.

LEREBOULLET, A. Mém. sur la Ligidia Persoonii Brdt. Annales des Sc. Nat., xx, 1843.

——— Mémoire sur les Crustacés de la Famille des Cloportides qui habitent les environs de Strasbourg. Mém. Soc. d'Hist. Nat. Strasb. Strasbourg, 1853. (Nervous system and brain of Oniscus murarius.)

LEYDIG, FR. Vom Bau des thierischen Körpers. Handbuch der vergleichenden Anatomie. Bd. 1. Tübingen, 1864. (Nervous system of Asellus aquaticus, Porcellio scaber and Oniscus murarius, Armadillo described.)

——— Taplen zur Vergleichenden Anatomie. Heft 1, Zum nervensystem und den Sinnesorganen der Würmer und Gliederfüssler. Tübingen, 1864. (Nervous system of Porcellio scaber; brain and eyes of Oniscus murarius figured.)

PAGENSTECHER. Phronima sedentaria. Ein Beitrag z. Anat. u. Phys. dieses Krebses. Archiv für Naturgeschichte, 1861.

RATHKE, H. Nueste Schrift. d. Naturg. Gesellsch, in Danzig, 1820. (Nervous system of Idothea.)

——— De Bopyro et Nereide. 1837.

RENTSCH. Homoiogenesis. Beitr. z. Natur- u. Heilkunde, 1860. (Nervous system of Gammarus ornatus.)

——— Nova Acta Nat. Cur. xx, 1843. (Aega, etc.).

ROUSSEL DE VANZÈME. Annales des Sc. Nat., i, 1834. (Nervous system of Cyamus, "beautiful figure" Leydig.)

STRAUSS-DURCKHEIM. Mém. sur les Hiella. Mém. du Muséum d'hist. Nat., xviii, 1829.

TREVIRANUS. Vermischte Schriften Anat. u. phys. Inhalts. 1816. (Nervous system of Oniscus and Cyamus.)

SARS, G. O. Histoire naturelle des Crustacés d'eau douce de Norvége. 1e Liv. Les Malacostraces. Avec 10 Pl. Christiania, 1867. 4°. (Nervous system of Asellus aquaticus.)

STOMAPODA.

BELLONCI. Morfologia del sistema nervoso centrale della squilla mantis. Annali del Museo civico di storia naturale di Genova, XII, 1878. (See also under GENERAL, Bellonci's Memoir on Squilla in Intorno alla struttura artropodi, &c.)

——— Nuove Ricerche sulla Struttura del Ganglio ottico della *Squilla mantis*. Mem. Acad. Sc. Inst. Bologna, iii, 419–426, Tav. 1–3, 1882.

CUVIER, G. Leçons sur l'anatomie comparée, 1809.

DELLE CHIAJE. Descrizione e notomia degli animali invertebrati della Sicilia citeriore. Napoli, 1841–'44.

SCHIZOPODA.

FREY. De Mysidis flexuosi anatomi, 1846.

——— and LEUCKART. Beiträge zur Kenntniss wirbellos. Thiere, 1847.

DECAPODA.

BELLONCI, G. Sui lobi olfattorii del Nephrops norvegicus. Memorie dell'Acc. d. Scienze di Bologne. 1880.

BRANDT UND RATZEBURG. Medizinische Zoologie, 1829.

CARUS, G. Lehrbuch der vergleichende Zootomie, 1818. Zweite Aufl., 1834.

CUVIER, G. Leçons d'anatomie comparée. Paris, 1809.

DIETL, M. J. Die Organization des Arthropodengehirns. Zeits. f. wissen. Zoologie, xxvii, p. 488, 1876.

HAECKEL, E. Ueber d. Gewebe des Flusskrebses. Archiv f. Anat. u. Phys., 1857.

HANNOVER. Recherches microscopiques sur le système nerveux, 1844.

HOME, E. On the internal structure of the human brain, when examined in the microscope, as compared with that of fishes, insects, and worms. Phil. Trans., 1824, Pl. 4, Fig. 3.

KRIEGER, K. R. Ueber das centrale Nervensystem des Flusskrebses. Zool. Anzeiger, 1. Jahrg., Nr. 15, 1878.

KRIEGAR, K. R. Ueber das Centralnervensystem des Flusskrebses. Zeits. f. wissen. Zoologie, xxxiii, p. 527, Jan. 23, 1880.

KROHN. Ueber die Verdauungsnerven des Krebses. Isis, 1834.

LEMOINE. Recherches pour servir à l'histoire des systèmes nerveux musculaire et glandulaire de l'écrevisse. Ann. des sciences nat. 5e sér. Zoologie, ix, 1868.

MÜLLER, JOH. Acta Acad. Cæsareo-Leopold., XIV.

NEWPORT, G. Phil. Trans., 1834. (Nervous system of Homarus.)

OFSIANNIKOFF, PH. Recherches sur la structure intime du système nerveux des Crustacés et principalement du homard. Ann. des sciences nat. 4e sér., Zoologie, xv, 1861.

——— Ueber die feinerer Structur des Kopfganglions bei den Krebsen, besonders beim *Palinurus locusta*. Mém. Acad. imp. sc. St.-Pétersbourg, vi, No. 10, 1863.

OWEN, R. Lectures on the comp. anatomy and physiology of the invertebrate animals, 1843.

PACKARD, A. S. Zoology for colleges, 1879. Brain of Cambarus and the blind *Orconectes pellucidus*, briefly described and figured. Fig. 269.

SCHLEMM. De hepate ac bile Crustaceorum, 1844.

SWAN. Comparative anatomy of the nervous system, 1835.

VALENTINE. Nova Acta Nat. Curios. xviii, 1836.

VIALLANES, H. Études histologiques et organologiques sur les Centres nerveux et les organes des Sens des Animaux articutes. Premier mémoire. Le Ganglion optique de la Langouste (*Palinurus vulgaris*). Annales des Sciences Nat. vie serie. Tom. xvii, July, 1884, 1–74.

WEBER, E. H. De aure et auditu hominis et animalium, 1820.

WILL. Archiv für Anat. u. Phys., 1844.

WILLIS. De anima brutorum, 1674.

YUNG, E. Recherches sur la structure intime et les fonctions du système nerveux central chez les Crustacés décapodes. Archives de Zoologie exp. et gén., vii, 1878.

——— De la structure intime du système nerveux central des Crustacés décapodes. Comptes rendus, lxxxviii, 1879.

MEROSTOMATA.

MILNE-EDWARDS, A. Recherches sur l'anatomie des Limules. Annales des sciences nat. Zoologie, xvii, Nov., 1872.

PACKARD, A. S., jr. On the development of the nervous system in Limulus. American Naturalist, ix, 422. July, 1875.

——— Structure of the eye of Limulus. Amer. Nat., xiv, 212. March, 1880.

——— Internal structure of the brain of Limulus. Amer. Nat., xiv, 445. June, 1880.

——— The anatomy, histology, and embryology of Limulus polyphemus. Anniversary memoirs of the Boston Soc. Nat. Hist. 1880.

VANDER HOEVEN, J. Recherches sur l'histoire naturelle et l'anatomie des Limules. Leyde, 1838.

EXPLANATION OF PLATES.

PLATE I.—ASELLUS COMMUNIS.

Fig. 1. Longitudinal section through the head on one side of mouth and œsophagus, showing the brain or procerebrum (*pcm*), first and second antennal ganglia; mandibular, first and second maxillary, the maxillipedal ganglia and nerves passing to the antennæ and mouth-parts × 1½ inch A.

Fig. 2. Section through the procerebral lobes in front of the optic nerves × ½ A.

PLATE II.—ASELLUS COMMUNIS.

Fig. 4. Section of the procerebrum posterior to Fig. 3, × ½ A.

Fig. 3. Section through procerebrum and main commissure × ½ A, 3*a*, ganglia cells from lobe *b*. × ⅛ C.

Fig. 5. Section through the median line of the head, involving the œsophagus and one of the procerebral lobes.

Fig. 6. Section through the head. × ½ A.

Fig. 7. Section of the head passing through one side of the first antennal ganglion and showing the origin of the first antennal nerve; also the second antennal ganglion, and mandibular ganglion (*md.g*) × ½ A.

Fig. 7*a*. Section passing near 7 and through the main commissure.

PLATE III.—ASELLUS COMMUNIS.

Fig. 8. Section passing through the main commissure from the procerebral to the 1st pedal ganglion.

Fig. 9–18. Horizontal sections from the top of the head downwards × ½ A.

PLATE IV.—ASELLUS COMMUNIS.

Fig. 19. Transverse section of the head through the procerebral lobes and through the eyes and optic nerves and commissures $\times \frac{1}{5}$ A.
Fig. 20. A section back of the procerebrum passing through the optic ganglion, optic nerve and eye.
Fig. 21. Same section as in Fig. 20, enlarged $\times \frac{1}{5}$ A, *rc*, retinal cells; *op, n*, optic nerve; *h, i, k*, masses of ganglion cells.
Fig. 22. Horizontal section through the main commissures and the first and second maxillary ganglia, and maxillipedal ganglia, and showing the origin of the mandibular nerves. $\times \frac{1}{2}$ A.
Fig. 23. The same section as in Fig. 22, enlarged. $\times \frac{1}{5}$ A.

PLATE V.—CÆCIDOTÆA STYGIUS.

Fig. 25. Transverse section through the procerebrum and commissures. $\times \frac{1}{2}$ A.
Fig. 26. Section a little posterior to that of Fig. 25. $\times \frac{1}{2}$ A.
Fig. 27. Enlarged sketch of section still farther back. $\times \frac{1}{5}$ A.
Fig. 28. Enlarged sketch of section still farther back. $\times \frac{1}{5}$ A.
Fig. 29. Section behind procerebrum and showing the rudimentary eye, but entire absence of the optic ganglion and optic nerve.
Fig. 30. Section through the eye. $\times \frac{1}{5}$ A.
Fig. 31. Section through the eye of another individual. $\times \frac{1}{5}$ A. *c*, lens. $\times \frac{1}{5}$ *c*.
Fig. 32. Section through a ventral ganglion.
Fig. 33. Section through a ventral ganglion.
Fig. 34. Section through a ventral ganglion under the stomach.
Fig. 35. Section through a ventral ganglion under the stomach.

NOTE.—All the figures drawn by the author with the camera lucida.

Fig. 2.

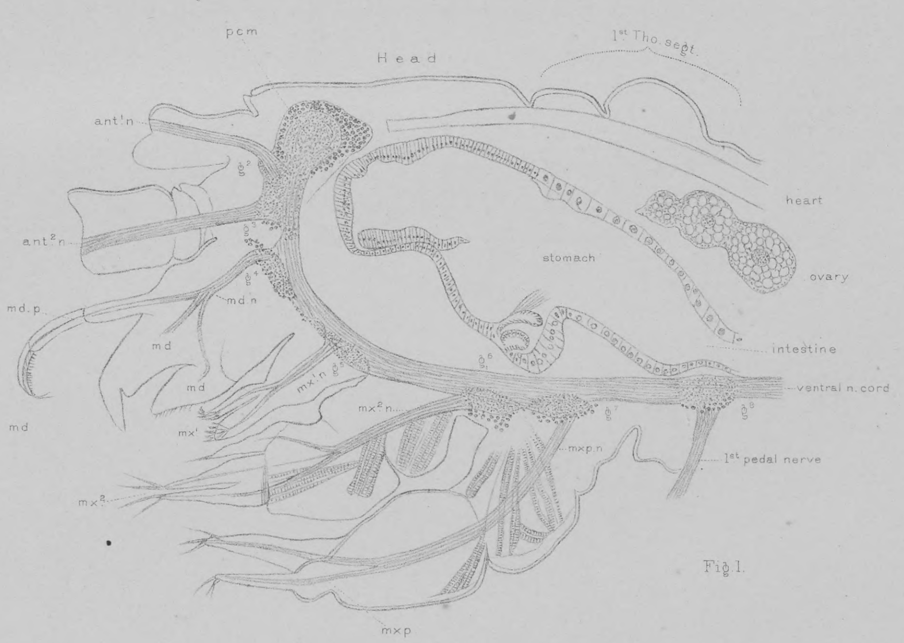
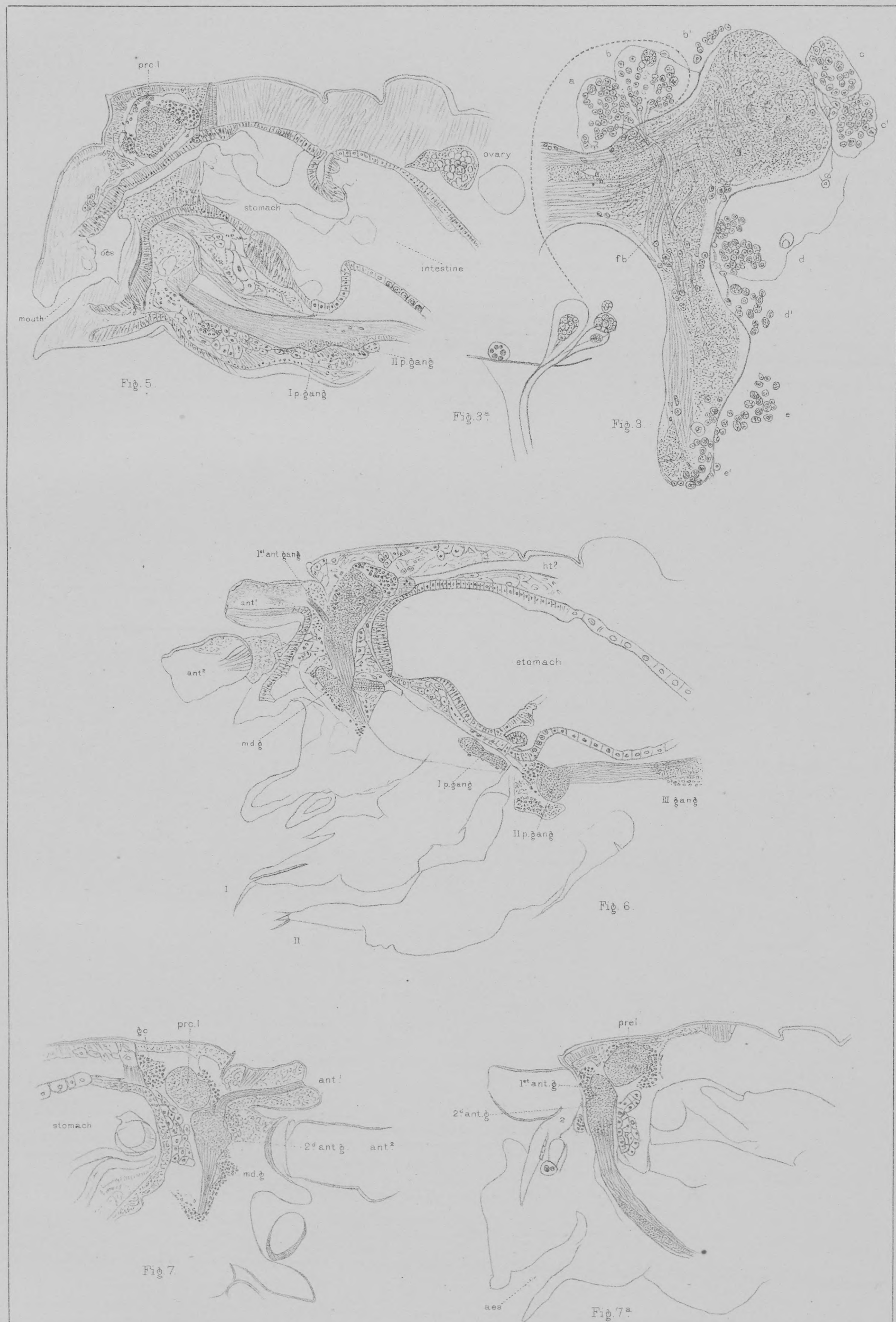


Fig. 1.

Fig. 4.

T. Sinclair & Son. Lith. Phila.



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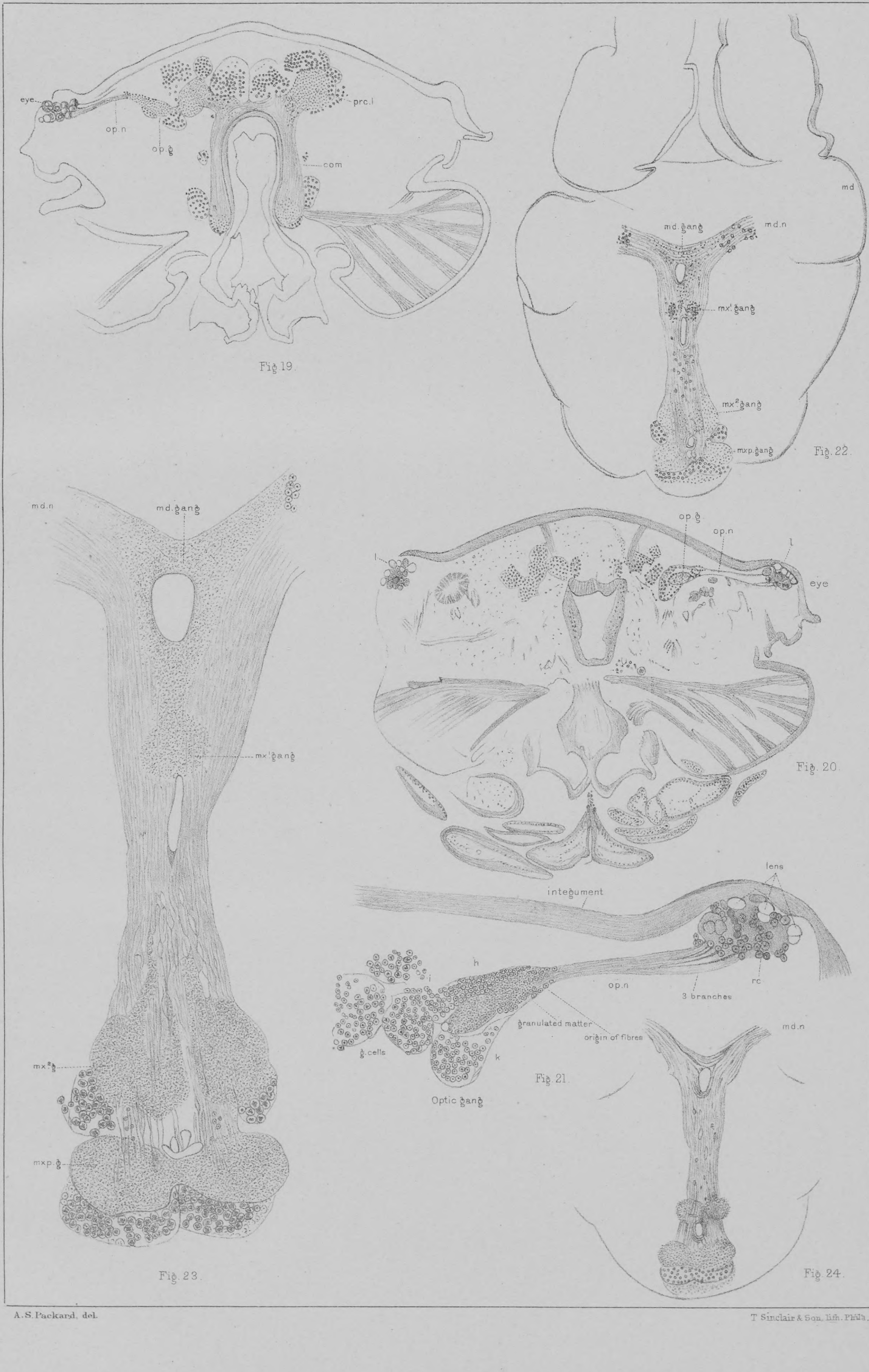

Fig. 19. Fig. 22. Fig. 20. Fig. 21. Fig. 23. Fig. 24.

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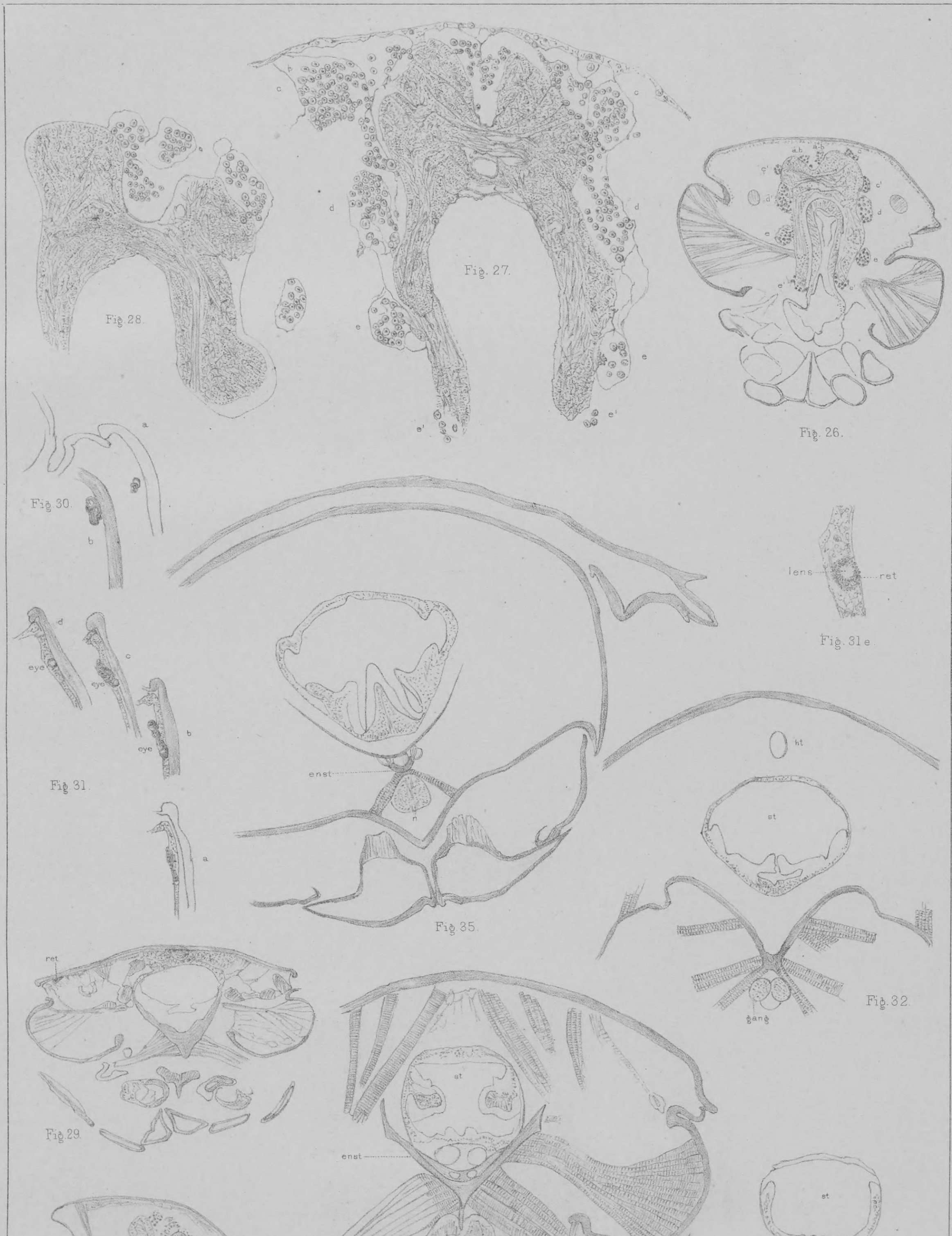
Plate V.



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T. Sinclair & Son, Lith. Phila.